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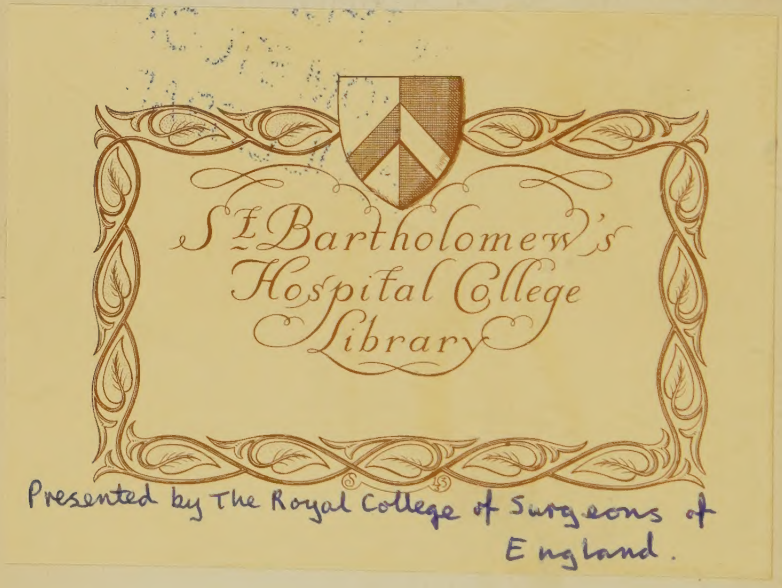

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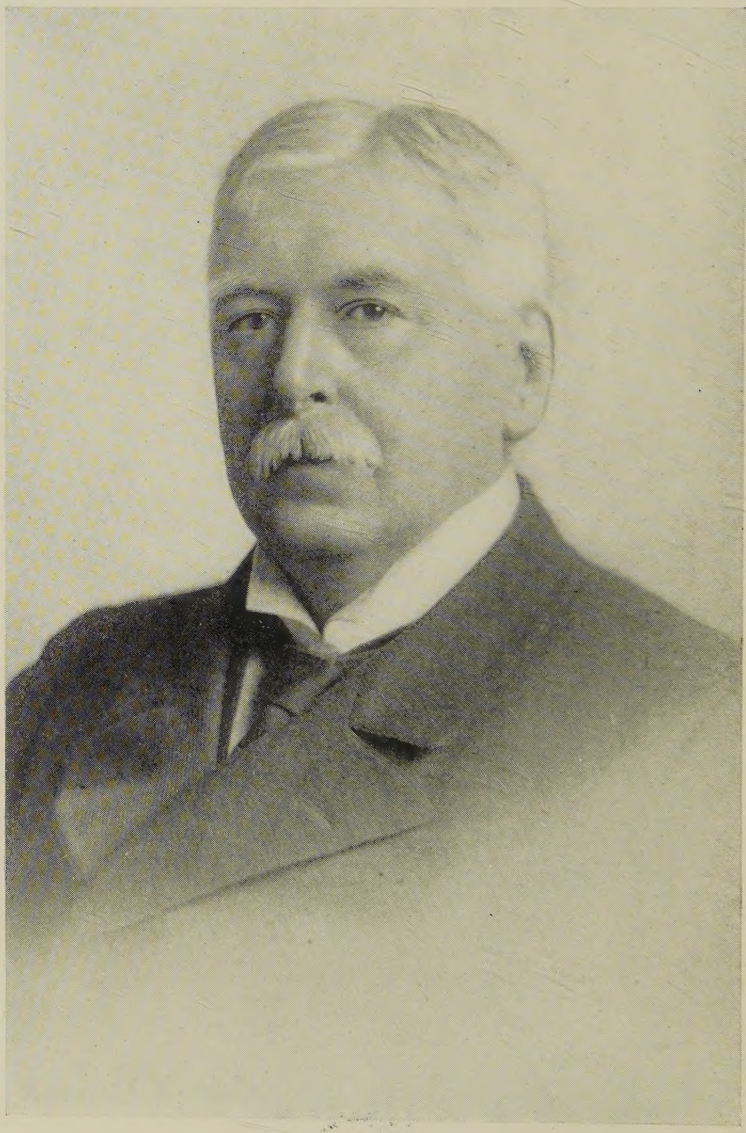

PLATE I.—Sir Patrick Manson at the zenith (1905).

THE LIFE AND WORK OF SIR PATRICK MANSON

BY

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WITH 12 HALF-TONE PLATES

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PREFACE

It is a truism that a man's character may be largely discovered in the records of his work—what he did, or helped others to do—and of the circumstances in which he did it. But a man does not reveal himself in work alone; a tale of work—howsoever original the theme and humane the purpose of the work—wants colour and sensible warm motion, even if, as in Manson's case, it does not lack diversity; and when a man has become widely known by his work, the world is curious to know something of his capabilities and discourse in the other occupations of life.

Of Manson, as a personality apart from his vocation, we have not—unfortunately for the curious—very much information until he was well into middle age. The flush of life—the vigorous expansive phase of early manhood, before forty-five, when (if ever) men are masters of their fates—was passed by him in China, in days when China was still a remote and rather taciturn country; and the record of his doings there is almost entirely one of earnest devotion to hospital and laboratory. We know that he was not submerged and sterilized in that work, since we have good evidence of a range of taste and sympathy far and wide beyond it in his inspiring speech at the inauguration of the Hongkong Medical College. What we chiefly get from the China records are impressions of a large, watchful, original mind, ever intent on the unknown or misunderstood causes of tropical maladies; and of a broad, humane spirit serenely exercised in faithful service to the diseased and deformed—often in the face of native prejudice and

sullen native obstruction. What we chiefly see is a great figure in a chosen calling

> "Benigne, and wonder diligent,
> And in adversitee ful pacient,"

but of whose conversation and sentiments and employments on matters away from his profession—of all those personal impressions and reactions that go to make up our "pleasing anxious being"—we are almost ignorant. It is not until, at the age of forty-five, he settled in London, that we get a full view of him as a man rich in a diversity of resources outside his varied interests in an interesting profession.

This abrupt break, or contrast, in the record has made it possible for two of us to participate harmoniously in the production of this account of his life and work. The principal author, whose name has precedence on the title-page, has dealt with Manson's fuller life after the migration to London, and is also responsible for the main fabric of the book, including illustrations and the collection of most of the original informative documents. The subsidiary author has contributed a connected story of Manson's work in China and of the mosquito-malaria hypothesis which was one of the natural issues of that work.

Our very cordial thanks are due to Dr. H. M. Hanschell, D.S.C., for having read much of the manuscript with a critical eye, and also to Dr. John Anderson, Professor of Medicine in the University of Hongkong, for having obtained for us a copy of the speech made by Manson on the day when the Hongkong Medical College—the germ of the present University—was brought into existence.

P. M.-B.
A. A.

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THE LIFE AND WORK OF SIR PATRICK MANSON

CHAPTER I

Early Life

Patrick Manson, the second son of a family of nine children, was born on 3 October, 1844, in the parish of Oldmeldrum, in Aberdeenshire, of a stock long settled in that county but of Orcadian descent—"Norwegian pirates" as in after years he would jestingly term them. His father, John Manson, was a man of standing in the parish, being laird of Fingask (an estate on the border of the town) and manager of the local branch of the British Linen Bank. His mother, a Blaikie and also of Aberdeenshire stock, seems to have been a woman of distinction, for besides possessing strength and beauty and good spirits, she had artistic talent and great power of resource. Patrick probably inherited much of her natural ability. It may be added that she lived to a good old age, and in her later years had a striking resemblance to Queen Victoria, which afforded her pride and caused much merriment. Cromlet Hill, where his parents kept house and where Patrick's earliest years were passed, still stands, solid and comfortable-looking, in the outskirts of Oldmeldrum.

In those early years Patrick was thought to be rather dull, probably because his youthful curiosity was satis-

fied chiefly by watching insects and frogs. One remembers how, at the same stage of life and in very similar circumstances, another of Scotland's famed philosophers, David Hume, did not gain a reputation as an infant prodigy.

In 1857 the Mansons moved into Aberdeen for the sake of the children's education and Patrick was sent to a school well known there as the Gymnasium, whence he went afterwards to the West End Academy. As a schoolboy he was studious, but was not considered brilliant. There is no mention of prizes, but it is on record that he was fond of cricket and did not like the society of girls, that he was skilled in the delicious sport of tickling trout and in the art of extricating hooked eels, and that in time he was accounted a good shot. So far we see the development of a healthy, happy schoolboy; but that the young Patrick must have been above the common run—that he must have been what the Scotch call "a lad of parts"—is evident from some other things noticed at this period of his life. For instance, he was—then, as always afterwards—very fond of carpentry, and could make good use of his hands. Again, having shot a lean and ferocious cat on his father's farm, he was profoundly interested in a tapeworm that he found while investigating its internal machinery in the seclusion of the house-top—a "prologue to the omen coming on."

His bent for carpentry and mechanics led to his being apprenticed, at the early age of 15, at the works of his mother's relatives, Messrs. Blaikie Brothers, ironmasters in Aberdeen. Here, though related to the head of the firm, he would claim no privileges, but worked along with the men, and kept the same hours, to show that he was as good a man as they. But the manly spirit

PLATE II.—Manson's birthplace, Cromlet Hill, Oldmeldrum, Aberdeenshire.

was too big for the boyish frame, and a curvature of the spine developed, and with it a paretic affection of the right arm that remained for life. His anxious parents took him to the physicians, with the result that he had to throw up work and become an invalid. The two hours daily that he was allowed to be off his back he devoted to the study of natural history at "the Marischal College of Aberdeen," and when at the end of five months the interdict against active work was removed, and he learned that the hours spent on natural history would count for the medical curriculum, he decided to give up engineering and become a physician.

Manson entered the University in 1860—the year when the two old Aberdeen foundations, "the University and King's College" and "the Marischal College and University," were merged into the one University of Aberdeen—though he did not pass his entrance examination until the year following, a not unusual custom in those days.

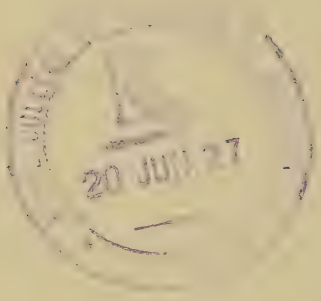
To supplement his education he also studied during the summer sessions at Edinburgh University, and so, at the end of four years, before he was twenty, he had passed his final examination. Being still too young to get his degree, he went to London and spent his time in reading, and visiting schools, hospitals, and museums; in short, in laying those broad foundations upon which he afterwards built so well.

At this time he is described by his friend and class-fellow Sir Alexander Ogston as a very strongly built young man, straight, and well proportioned, in spite of the curvature of the spine, with a thin and particularly handsome face, marked by a certain good-humoured satirical expression. In his hours of relaxation he delved into English literature and, in common with his friends,

was especially attracted by Christopher North's "Noctes Ambrosianæ," from which he used to quote freely.

In October, 1865, shortly after reaching the statutory age of 21 years, Manson graduated M.B., C.M. of Aberdeen University, and very soon afterwards was appointed Assistant Medical Officer to the Durham Lunatic Asylum, a post which he held for seven months. There he appears to have availed himself freely of facilities for post-mortem examinations, and, in his own words, had "ample opportunity of finding out how crassly ignorant I was and how terribly inadequate the medical training of those days." That he made good use of his opportunities is shown by his first paper, published early in 1866, on "A Peculiar Affection of the Internal Carotid Artery in connexion with Diseases of the Brain." He writes: "Any fact, however obscure its bearing on the subject, in connexion with disease of the nervous system, is at present considered valuable." In all of seventeen consecutive post-mortem dissections he had found a peculiar aneurysmal—sometimes fusiform—dilatation of the internal carotid, most marked in general paralytics, in the aged, or in any case associated with considerable atrophy of the brain. From this series of observations he concluded that "just as in cases of cirrhose atrophy of the liver the obstructed portal circulation relieves itself by an ascitic effusion, or rupture of the intestinal capillaries, so the obstructed circulation through the brain relieves itself, or rather results in a dilation of the internal carotid, the ascites in the one, and the dilation in the other, both being the effects of mechanical pressure on the walls of the vessels." This paper was offered as a thesis for the Aberdeen M.D. degree, which was conferred upon him in July, 1866. According to Sir Alex. Ogston, the careful dis-

PLATE III.—Patrick Manson in 1864 (*ætat.* 20).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sections on which it is based are still preserved in the University Museum of Anatomy.

Even in this early paper he displays his remarkable aptitude for observing all facts with an equal mind and then for inquiring into their causation and interpretation.

Manson very soon tired of mental work within the confines of an asylum, but his adventurous spirit found an opening when, through the interest of his elder brother, then in Shanghai, he obtained the appointment of Medical Officer for Formosa to the Chinese Imperial Maritime Customs, a service at that time under the direction of the late Sir Robert Hart, who recruited most of his medical officers from the Scottish Schools. This appointment decided his career, and he left his native home, not to return for nine years. His first contact with an indigenous tropical disease appears to have been at Tamatave, in Madagascar, where his ship touched on its way out viâ the Cape. In the mission hospital there he was shown a number of coolies suffering from various degrees of cardiac failure, supposed to be due to disease; but on visiting the wards next day, he found that many whom he had considered to be in imminent danger of death had recovered sufficiently to leave their beds and perambulate the wards. Though he then distrusted the diagnosis of valvular disease, it was not till many years afterwards that he realized that what he had seen was beriberi.

It was on a dark night in June, 1866, that Manson was set down with all his belongings at Takao, on the south-west coast of the island of Formosa, where, in a small society of about sixteen Europeans, he was to have his home for nearly five years. His official duties were to inspect the ships calling at the port and to

treat their crews, and also to keep up the meteorological record, since Formosa is a nursery of typhoons. Outside the official routine he had his private practice among the Europeans and Chinese, and there was also a native missionary hospital that invited his daily attention. He also made frequent travel in the surrounding country, to observe the endemic diseases, and particularly elephantiasis and leprosy, and he often went as far as the mountainous backbone of the island, where he got to know the aborigines—*Sheng-fan*, or "wild men"—and learned their language. For recreation he had riding and gardening, and bathing and fishing in the lagoon that intersects the settlement.

Isolated from his profession, and not provided with apparatus other than a simple lens, a young man can hardly attempt discoveries in tropical medicine. But Manson did better in the circumstances. True to his maxim that in a novel environment all facts are of equal value, he kept careful notes of all his observations.

Here, for instance, is a reference to beriberi, a disease which, knowing nothing of it but its name, he did not recognize :

"One day I was called to see a Chinese clerk employed in a European firm. I had known him well as an active, obliging young fellow. I found the patient sitting propped up in a chair, short of breath, dropsical from head to foot, with a cardiac bruit, irregular tumbling action of the heart, and complaining of a feeling of distress in his chest. As there was no albumin in his urine, and as there was a loud bruit with manifest disturbance of the heart, I felt convinced it was a case of heart disease, and, from other circumstances in the case, one which treatment would benefit. I prescribed and went away feeling, and probably expressing by my manner, that we Europeans know a great deal more about disease than the old Chinese doctor, who hitherto, together with the village idol, had been treating the patient. Next day I went

to see the lad expecting to find an improvement. I thought as I passed the door that there was a strange hush about the house. I entered what was my patient's room. On the bed, covered with a blanket, there was something long and rounded and still, which explained the hush I had noticed; and so I left the house sadder and wiser, and, I trust, humbler than I did the day before."

Takao is only just within the tropic, its climate is not distressing, and except in the last year, when he began to suffer from attacks of that gout which obstructed his activity so greatly all the rest of his life, Manson enjoyed good health there; though, in common with his European patients, he became acquainted with the condition known as "tropical liver" when ships having stores of beef and port wine arrived from England. He was prosperous too; so that at the end of his third year, at the early age of 25, he was able, with honest pride, to repay his father all the cost of his medical education—£700.

Manson's connexion with Formosa was rather abruptly broken at the end of 1870. The island had been long in a state of unrest, and as he happened to be on friendly terms with some native contractors who appeared to be abetting one of the local Chinese factions, he was advised by the British Consul to leave it; and so, early in 1871, he set sail for Amoy.

CHAPTER II

At Amoy: The General Outlook

Amoy, where Manson led a busy life during the greater part of the next thirteen years—until December, 1883, is a large Chinese town lying on the southern slopes and foreshore of one of the numerous islands in the Bay of Hiu Tau, just about one degree north of the tropic. As the port of the big inland city of Chang Chow it had a large trade, and, as a Treaty Port, a considerable European population (at that time about 150 persons) and fine accommodation for European shipping. The European quarter, where Manson lived, is situated on Koolongsu, a rocky little island quite close to the town. Here the climate, tempered by the diurnal sea-breeze, is not unpleasant, and, indeed, must have been subject to some surprisingly invigorating intermissions for a place lying in nearly the same latitude as Muscat, since in Manson's first winter "ice, hitherto known to the inhabitants by hearsay only, was actually seen for three or four mornings in mid-December." Manson's opinion was that "for Europeans, as they are housed, the climate cannot be considered unhealthy. Did the residents display as much wisdom in the furnishing of their table as they have in the building of their houses, they might live as comfortably here as they could in Europe. The inevitable sherry and bitters, brandy and soda, combined with want of exercise and

PLATE IV.—Amoy Harbour in 1873, from Koolongsu.

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a high temperature, induce disease which is hardly climatic, although the victim may call and think it so." Of the native city, on the other hand, all that he could say was that "the streets, narrow and irregular, are filthy and redolent of every impurity. Pigs and dogs are the sole representatives of the elaborate machinery of sanitation in use in European towns. But, there is no typhus, no typhoid or other diseases considered the inevitable consequence of defective sanitation."

The people of Amoy, though industrious, were poor in the extreme. Rice, or rice mixed with sweet-potato, flavoured with vegetable pickle or salt fish, was their staple food, pork and beef being the luxuries of the few. Some were even too poor to afford fire-wood for cooking, and had to be content with sweet-potato softened in water and seasoned with a little salt begged from a charitable neighbour.

It was in these unconsidered surroundings that Manson, by a happy inspiration all his own, made one of the most prolific discoveries in the pathology of tropical disease.

As Medical Officer to the Imperial Maritime Customs Manson had medical charge of the shipping, and a hospital for European seamen; among his unofficial activities was included the medical charge of a Missionary Society's hospital and dispensary for the Chinese to which he devoted a great deal of his time and thought.

Great men, as a rule, are not given to writing about themselves, and so we know very little of Manson's life at Amoy except as it is revealed in his work. We have to picture him as a very busy man—sometimes, as he said, almost too busy to sleep; but not as a man who scorned delights and merely lived laborious days. On the contrary, he took full advantages of the good

opportunities of sport offered on the neighbouring mainland, and was reputed to be one of the best snipe-shots in China; and when a tiger-shoot was arranged he was keen to join in it. Once a year or so he made a leisurely trip up the Kiulung river into the interior and coupled sport with exploration. Fortunately Dr. H. B. Guppy, F.R.S., has given us a vivid reminiscence [1] of his general bearing and appearance after more than six years of Amoy. "There was," writes Guppy—looking happily back on his flying visit to Amoy, as Surgeon of H.M.S. *Hornet*, in August, 1877—"a spirit of service amongst the principal official and professional associations of Britishers at the Chinese Treaty Ports in the seventies of last century. . . . The medical representatives gave their best for the good of the land in which their fate had cast them. Of such a type was Patrick Manson, working with his microscope amongst his Chinese patients in his private hospital at Amoy, his appearance of health and vigour more suggestive of a British hunting squire than of a student on the eve of a discovery that has wrought so much for the benefit of mankind."

The work with his microscope, among his patients, in which we see the master mind searching for the causes of the then little understood diseases that he had to treat, was only one episode in the daily round. There was also a great deal, and a great variety, of purely professional work to be done, and we know that Manson went to it with equal enthusiasm, since all his life he was conspicuously one of those who believed it to be a doctor's first concern to heal the sick. We know, further, that he had a keen desire to impart knowledge

[1] *Transactions of the Royal Society of Tropical Medicine and Hygiene*, 1925, xviii. 386.

and to utilize his hospital towards improving the standards of the native Chinese practitioner. Fortunately we have a very full record of his activities in all these directions; but before exploring the record it is necessary to have an idea of the peculiarities of the human environment in which the work was done, and of the modest machinery at the worker's disposal. Only then can the results be appreciated to the full. And even then it has to be considered that Manson was often single-handed; for although missionary doctors might visit their hospital from time to time, and although two not very successful experiments were made with assistants, the only really critical assistance he received was from his younger—and favourite—brother David, who, after a brilliant academic career at Aberdeen University, joined him in 1873. David, however, was appointed medical officer at Takao in 1875,[1] and had to leave him then.

To speak, first, of the human environment in which Manson did most of his work. Of the Chinaman in the ordinary tenor of his way Manson thought well: he found him kindly, industrious, and in many ways profoundly interesting. But, like all Orientals, the Chinese were suspicious of Western medicine, and particularly of Western physic. At the time of his arrival in Amoy they were greatly disturbed by rumours of a poisonous pill, *Shan-si-fan*, said to be distributed by Europeans, and one of the early signs of budding confidence in his hospital was a belief that on application there and merely giving one's name—alluding to the entry in the hospital register—the antidote would be

[1] David Manson died of sunstroke when playing cricket at Foochow in 1878. After his death the hospital at Takao was called by his name as a memorial of him.

supplied. Later on, when he proposed surgical operation for the removal of those elephantoid tumours which at Amoy—as in many other parts of the tropics—are one of the commonest as well as one of the weariest and most lingering burdens laid on man, it was by the merest good fortune that he hit upon an initial case that enabled him to shake the native distrust of a proceeding so dreadful. The story may be given in his own words:

"The first patient was but 19 years of age, who had been attacked by the disease, which had almost completely immobilized him for fully five years. Soon he was unable to work properly and, finally, the mass became so enormous as to make work almost impossible. He was thus thrown on his family for support. A brother became the sole breadwinner, and grumbled at having an additional mouth to fill and no one to help him. Often he was reproached by his brother with his uselessness, his repulsive appearance, and it was even suggested that the patient should make away with himself. At last, the brother succeeded in driving him in despair to adopt this unnatural suggestion, and twice he attempted suicide by swallowing arsenic. Both attempts failed; the dose taken, being probably too large, was quickly got rid of by vomiting. He had heard that in the Chinese hospital at Amoy, the foreign surgeon attempted the removal of tumours by the knife, and failing to kill himself, he thought he couldn't do better than let another kill him, or perhaps by good fortune remove the disease that caused him so much misery. It was in this frame of mind that he presented himself at the hospital, perfectly prepared to submit to anything and no persuasion required, an excellent illustration of the peculiar philosophical resignation with which a Chinaman submits to the inevitable. As was the rule before undertaking a dangerous operation, he was warned of a possibly fatal result; to this he was perfectly indifferent. He would say, 'I have tried to kill myself twice but failed; if you succeed in curing me so much the better, but don't let the consideration of my death deter you in anything you care to do. I am not afraid of death.'"

The operation was performed after the patient had been prepared for it by about a month of good treatment

in hospital, and the tumour, which (when subsequently drained of fluid) weighed 7½ lb., was removed. Had the patient succumbed—and so soon after the *Shan-si-fan* rumour—the result would have been fatal also to the growing repute of the hospital; but fortunately he made an excellent recovery. It may be added that some months afterwards this good young man went to Koolongsu to look for Manson. With tears, as he met his benefactor, he held out a dollar to view: he had earned it in Amoy since his restoration to active life: he wished to give it to the hospital as part payment of his debt, but—but his home was far off, and—and he was suddenly recalled there by the death of a near and dear relative. Manson, of course, took the hint and gave the man his dismission, but was not the less pleased with this touching show of gratitude because it was enacted in Oriental fashion, being content to have gained a zealous convert.

The native practitioners could hardly be expected to discourage the popular superstition. Thus, when for the first time in Amoy Manson performed the impressive operation of lithotomy, although some of these virtuosi did seem awed with a diagnosis so correct and an operation so precisely anticipated and so exactly executed in all its successive steps, yet one of them stated boldly that an operation was quite unnecessary since he could remove the stone, and without any risk to the patient, by medicines alone. The patient in this first Amoy case of lithotomy was a young man of 18 years, whose troubles had existed from his youth up, as far back as he could remember, and who was now worn to a shadow with continual and acute suffering. In this case also the result of the operation produced another zealous witness to the trustworthiness of the British surgeon; for about a month afterwards he paid a visit to the

hospital, bringing some friends with him, but until he had introduced himself Manson could hardly recognize, in the fat smiling face, the emaciated sufferer he had been familiar with five months before.

The gratitude evinced by regenerated patients was not always so spontaneous and exuberant as that shown by the two youths described above. For when lithotomy was becoming a familiar procedure, it was suggested to a man who had been relieved of a large stone, and was discovered during his convalescence to be worth about 3,000 dollars, that, since he had been cured of an exceedingly painful disease and restored to a happy life, he might give something to the hospital in return. After a week's deliberation the man informed Manson that he had decided to present the munificent sum of one dollar!

Though, as will be shown more fully later on, Manson managed in the course of time, and with occasional intermissions, to lull, if not to extinguish, the suspicious element of the Chinese character, there was another element—the "Heathen Chinee" aspect—which sometimes kept the course of European medicine from running smooth, as is illustrated by the following story.

One day, going his rounds, Manson was asked for his discharge by a newly admitted patient who had come in from a very long distance for treatment. Manson expressed surprise and asked the reason. With great reluctance the man at last informed him that on reaching Amoy he had been swindled out of all the money (three or four dollars) that he had provided for his expenses by a stranger who had introduced himself as a servant of the hospital, whose duty it was to provide intending patients with two pills to ensure the efficacy of their prospective treatment. These the impostor gave to the

patient, taking from him all his money. The victim was sent back by Manson to the scene of his misfortune, where he soon found the swindler, and tried with threats to get his money returned. Threats failing, he attempted to bring the rascal to the hospital by force. But a sorcerer who also practised in the neighbourhood interfered, and as this personage was well known to be in collusion with the Head Man of the district, the bystanders, who had hitherto assisted with their sympathies, drew off, and so the patient had to return to hospital empty-handed.

Manson, with a wise head on his young shoulders—he was but twenty-seven when he started in Amoy—and with a heart native to the head, was not perplexed or provoked either by rumour, "Stuffing the ears of men with false reports," or by impostors. He made allowances; he put himself in the Chinaman's place; and he has left the following reflections on the subject.

He realized that mistrust of operative surgery was being overcome. As he said, the detection of a stone concealed within the body, the dramatic precision of the steps by which the evil thing is approached, the triumphant extraction from the hidden depths of the wound, and the quick change from misery to ease—all this, impressive enough to the educated man, is to the ignorant a convincing miracle. But, as he went on to say, with medicine it is otherwise. The physician, assisting nature by an art that appeals only to the instructed judgment, has no theatrical results to display to the ignorant: "and since most people recover from ordinary sickness without treatment, or even in spite of treatment, whether by European or by Chinese doctors, the Chinaman can point to plenty of cures as attributable to his own native medicines and methods. He, there-

fore, sees no reason in giving up the old familiar ways, in which he has faith, for something new and strange: he is prudently conservative in this matter and is worthy of respect on that account." But these generous opinions must have seemed rather reactionary to the indignant Europeans who could not persuade their Chinese servants and subordinates to give up their contemptible potions and lotions. In his posture towards Western medicine the incredulous Chinaman sought for a sign, and the sort of sign he expected is revealed in the following exquisite story, with which Manson enlivened the pages of the *Customs Gazette*.

" One morning, very early, a Chinese stranger entered a chapel belonging to the London Missionary Society and seated himself, evidently with a purpose, on one of the benches provided for the audience in the preaching hall. Supposing he came to inquire about the doctrines of Christianity, a native preacher proceeded to expound the Scriptures to him, patiently lecturing all through the morning. Breakfast-time came, and still the apparent inquirer showed no disposition to move, but continued to listen to the instructions of the preacher. Dinner-time, afternoon, supper-time, all passed, but the lecture was continued without a sign from the stranger of an intention to move. At last, the preacher, supposing he was going to be saddled with a guest for the night, became impatient and, suspecting the honesty of the stranger, asked him what he came for. Many inquiries failed to elicit a satisfactory answer. The idea then struck the preacher that his would-be guest (he had heard of such a thing before) had come to be under the protecting influence of the God of the chapel, that He might, through His antagonism to the devil of ague, avert an impending ague-fit. And such really was the

case. Nor was the invalid disappointed; his belief had really made him whole. On inquiry it turned out that the chapel enjoyed a great reputation, many resorting to it for that purpose." In this case the Chinaman was doing better than he knew, for "the effect of emotions such as fear, rage, etc., in preventing the development of an ague-fit is well known. That the ague of a Chinaman, with his credulity and superstition, should be very amenable to such influences is what from his nature we should anticipate."

Having in these various ways looked into the mind of the ignorant Chinaman and there found ideas of medicine and physic of much the same complexion as those of the vulgar in all parts of the Earth, Manson looked round about and noticed that the Chinese doctor, like the proverbial conjuror, emphasized the circumstance that there was no deception about his wonders by doing all his work in the open street. "He feels his patient's pulse, advises, and prescribes, usually in an open shop, where every passer-by can see what is going on and every prospective patient know what he may expect." The European doctor, on the other hand, saw all his patients in the innermost chamber of the top of a three-story house, thus giving the freest play to the trepidation of the general public, who never ventured near the secret and remote consulting-room. It was here that Manson saw a line of reformation and a hope of progress. "Our work," said he, "is good and honest, and nothing should be left undone to make this generally known. We may not be able to prevent the malevolent and the gossips detracting from our character, or thieves and impostors swindling in our name; but we can by a very few simple arrangements make the real nature of our work evident to the public

and so do something towards counteracting the evil influences I have spoken about." As a result of Manson's sensible observations the consulting-room and dispensary were removed from the top story of the hospital to the ground-floor, facing the street, so that passers-by could see and hear what was going on there; moreover, a separate waiting-room was provided for women.

Surgery might have its triumphs and medicine its prospects, but nothing could overcome the bitter antipathy of the Chinese to that third and most instructive phase of hospital experience—the examination of the body after death. This was a great hindrance to Manson, particularly when he began his work on the *Filaria* and wanted to follow and find the adult worm in the lymphatics of the human host. Thrice only did Manson start on this expectant field, and then the reaction upon the populace was such as to set a final interdiction on it as an open proceeding in Amoy. The subject in this case was the body of a Chinaman which, it was believed, might harbour an adult *Filaria*. Just before the man's death it had been agreed that a sum of two hundred dollars should be paid to his widow for permission to do a simple but adequate dissection of his body, without removal of any part of it. The bond had been sealed before death, and the widow had received the money afterwards. It was a broiling day when Manson and his brother David took their way to the dead man's house, in the heart of the native town, to complete the transaction "nominated in the bond." The room where the body lay was exceedingly hot and admitted no air and little light. Soon after operations were started a mob gathered outside, curious to know what the "foreign devils" were doing, and the outcome

was that the " foreign devils " had to run for their lives, and that Manson got nothing for his investment. Another post-mortem of a simpler sort, but managed with more secrecy, and with stealthy native aid, was less exciting. He imagined that some sudden deaths were due to trichiniasis and was determined to settle his uncertainty. " I always entertained," he said, " a great respect for what we in our civilized pride call ' native prejudices ' ; but in this instance, in view of the importance of correct diagnosis, I thought I would run the risk of offending these for once. So with the aid of an old and seasoned native porter I smuggled one of the dead bodies into a piece of waste ground near the hospital, and there behind a wall excised a piece of muscle to look for trichinosis. Needless to say, I found none. In this way I groped about for a long time in search of a diagnosis. It was not till months had elapsed and not a few deaths had occurred that I recognized that I had to deal with an epidemic of beriberi, and it was only then that it began to dawn on me that the poor dropsical fellow who had died in Formosa some years before [*see* p. 6] had really died from beriberi and not from heart disease after all."

To conclude the survey of the surroundings in which Manson worked, and of his posture in them, something must be said of the hospital scenery, and particularly of the missionary hospital. For the first three years of his charge this hospital was merely a Chinese house, and much overcrowded; but in 1874 the patients were transferred to a properly designed building. Its clientage was by no means all of it poverty-stricken, and when Manson was quite sure of this fact he expressed himself sensibly on the absurdity of providing free treatment for every one who cared to ask for it—a

practice that attracted the greedy bourgeois, and repelled the proud mandarin whose goodwill was so desirable. If, as he said, the medical science and art of the West are expected to be appreciated, they must be represented as something worth paying for by those who have the means. The Emperor of China did not realize the value of a railway that was given to him for nothing. It is all very well, said Manson, to give the gratuitous method free play when introducing a new system. Let us at our own expense show the Chinese a good thing in working order, then let such of them as want the same thing, and can afford to pay for it, get it in the usual way. He illustrated his argument by a characteristically homely figure. "Quinine is as valuable to a man shivering with ague as a piece of grey shirting; and when he knows this he will ask for it, and get it. But if grey shirtings were only to be got from one or two charitable individuals, either the mass would probably remain in ignorance of their existence, or the charity of these individuals be soon exhausted. Private enterprise would be choked by the give-away-for-nothing system of the philanthropist, and certainly clothes for the million would not be forthcoming. Left to the wholesome influence of demand and supply, we know how marvellous has been the result, and so it should be with quinine"—and with medical advice. Manson therefore opened, in the centre of the town, a consulting-room, a dispensary, and a small private hospital, where he received patients who could afford to pay for advice and treatment. Here very soon came the mandarins and rich merchants, the first patient being one of the principal druggists of Amoy, in whom, and to the friends who accompanied him, Manson was able to demonstrate the presence of a calculus. This cautious

man, however, would not consent to its removal until he had applied for his mother's blessing and also had watched Manson perform some operations—one of them a propitious lithotomy—at the native hospital, and had observed the results.

The staff of the mission hospital when Manson assumed charge consisted of a native house-surgeon and two dressers, and when the two last left he encouraged other young Chinamen to come in, taking for the most part Christian students recommended by the missionaries. Manson has not recorded his opinions of missions and missionaries; but we may assume that the founders of this hospital must have included some of those free unprejudiced souls who understand that where the projected mission-field lies in a land possessed of an ancient civilization and a not ignoble philosophy of its own, the missionary should be not so much the rigid mouthpiece of a dogmatic theology as a man who, without any direct proselytism, breathes the spirit and shows forth the example of Chaucer's good parson:

> "First he wroghte and afterward he taughte;
> To drawen folk to Heven by fairnesse
> By good ensample was his bisinesse."

They must have understood that when a man of this type is also skilled in medicine and surgery he makes a mission hospital—or any hospital in any land—one of the most persuasive and pervasive influences in spreading the true spirit of Christianity that can well be imagined. Manson's thoughts at Amoy did not, as we have seen, show even a tendency to run in this direction. He had, it is true, the missionary spirit in overflowing measure; but it was as a missionary of

Western Medicine that he accepted himself. He therefore made the fullest possible use of the hospital as an instrument of medical education, and his class of students increased year by year. His hope, like that of the gospel missionary, was that the young men whom he trained in medicine would go forth and settle in the neighbouring province and there spread abroad the light of what he had taught them. It was in exactly the same spirit that, many years afterwards, he set to work to start the School of Tropical Medicine in London. Like a gospel missionary, too, he translated into the vernacular, with the assistance of his house-surgeon, Dr. Kuitsong, what he considered to be the completest and most comprehensive book on a subject which from his own experiences he knew to be the one most appropriate to the particular circumstances of the Chinese practitioner.[1] Unconsciously—and all the better for that—he was all the while doing exactly what the original founders of the mission hospital must have had in mind, for no one who has ever seen Manson at work among his patients or his students at the Albert Dock Hospital, clothed with benign sympathy as with a garment, can have any doubt as to what his spiritual influence was and must always have been. But, unfortunately, in the ninth year of Manson's régime at Amoy there arose a missionary of the rigorous proselytizing "hot-gospeller" species, who convinced himself that in Manson's competent hands the mission hospital had become merely an appanage of a medical school and had ceased to be an "agency for the spread of the gospel," and this

[1] This was Curling's "Diseases of the Testis." To his translation he added a chapter on the anatomy of the specified region and an appendix containing a description of his own method of operation for elephantiasis of the scrotum.

worthy almost persuaded the missionaries to relinquish their interest in the place.

Friction with the missionaries was not the only check that threatened Manson's scheme of medical education. There came some temporary difficulties in finding openings for the trained students. Manson had foreseen this, and at the outset of their course had told these young men that their future depended entirely on themselves, that they would not be propped up and petted all their lives, and that before they could start in practice they would have to provide themselves with instruments and drugs. Nevertheless, when the crisis came they seemed to have some idea that they could fall back on the familiar paternal hospital for assistance, and the result was that Manson himself had to set them going with loans. These seem to have been repaid to him, according to promise, within a reasonable time, since there is no record to the contrary. It may be assumed that the young men succeeded in after life, since it was one of the worthy missionary's grievances that they had "ambition" and that a "good few" of them had "established themselves throughout the country"; moreover, Manson stuck to his point that his scheme "contained the germs of great results."

In this chapter we have endeavoured to show something of the Amoy stage and its scenes and scenery, and of how the actor took his various cues there. In the chapters next following we propose to deal somewhat in detail with the work done.

CHAPTER III

At Amoy: The Medical and Surgical Field

In a retrospect of Manson's work at Amoy it has to be borne in mind by home-keeping youth that he was cut off from all the incentives that incline the ordinary man to take up original work. There were no libraries or museums, no lively scientific meetings, no emulous colleagues; he had not even the traditions of a big Medical Service as a solace. Moreover, his professional education had been almost entirely provincial, and he had left England too soon and too untaught to have formed any personal ties with some of those Bandusian fountains of pure science which mean so much to a young man in need of technical guidance and tutelage. So that even when, mainly by the light of Nature, he had hit upon facts that struck him as novel, or as having some general interest, there was, at the outset, no mouthpiece for him better than a *Chinese Customs Gazette* or a Missionary Report—where it had small chance of being noticed at all, much less of being critically discussed in Europe. It is hard in these days of travelling scholarships and fellowships, and of medical research laboratories from Jerusalem to Madagascar, to realize a position more isolated, a situation where a young man of promise is more completely thrown upon himself for the means of following up his ideas and cultivating his opportunities. Perhaps only those who have served in

situations in some respects similar can understand how solid the obstructions are, and how high must be the natural endowment of a man who overcomes them so effectively as Manson did.

In this chapter we are to follow Manson's goings-forth as a medical practitioner in Amoy; and we begin with his doings as a surgeon, because it was by his surgical skill that he began to gain the confidence of his reluctant Chinese patients, and—above all—because it was his early engrossment in a particular surgical province, to wit, the treatment of elephantiasis, that took him through unknown tracts on to the famous and far-reaching discovery for which, in Professor Blanchard's memorable phrase, he was acclaimed, after the passage of thirty-five critical years, "Father of the modern science of Tropical Medicine." But this discovery, and the light that it shed on elephantiasis, will be the theme of the next chapter.

Of all the diseases—smallpox, syphilis, leprosy, malaria, and what for the moment must be called merely "elephantiasis"—with which Amoy was saturated, one of the most distressing in its consequences was this elephantiasis, the ultimate nature and the causation of which were in 1871 quite unknown. It obtruded itself upon Manson's notice chiefly in the form of scrotal tumours, which might attain a weight of more than fifty pounds, and, as has been already told, might make an intelligent and conscientious man a hopeless burden to himself and his family. What Manson had first to do was to devise a safe surgical procedure for these sufferers, and to persuade them to undergo it. His first operation—strikingly successful, as has been related—worked like a miracle and was soon followed by eleven more cases, all having a happy issue; in one of them

the tumour had been of such size that the patient, *ex necessitate*, was accustomed to use it as a "not very elegant substitute for a table" when squatting at meals. Then there occurred an ominous check, which was traced to a slanderous rumour that half the patients operated upon had died as the result. Manson's urbanity soon overcame this check, so that in the course of the first three years sufferers in Amoy were relieved of some hundredweights of burdensome elephantoid tissue, the record of the first sixty-one cases including an individual tumour of 32 pounds, and only two deaths as a consequence of operation. Another diseased state, cognate with elephantiasis, for the radical cure of which Manson frequently had to operate was lymph scrotum. In other directions his everyday run of surgical practice included lithotomy, and, after 1876—when in the course of a year's leave in England he had acquired some experience at Moorfields, under Nettleship—operations for cataract and other forms of eye disease.

Manson used to speak of himself with characteristic moderation as "an indifferent surgeon but a good carpenter"; but if we may judge from some casual statistics published in reports of the mission hospital, he made a very different impression upon his Chinese fellow-citizens. The figures show that in his first year (1871) the number of patients that received attention was 1,980 (of which one-fourth were in-patients), and in 1874 had increased to 4,476; and that the number of operations, over a wide field of surgery, which in 1877 was 237, had in 1879 increased to 379.

In his varied practice among European seamen and others Manson had considerable experience of the surgical treatment of liver abscess, which in those days, before the introduction of the aspirator and the general

PLATE V.—Patrick Manson in 1875 (*ætat.* 31).

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diffusion of Listerian doctrine, was still accounted a hopeless disease; and he formulated the results of it in a paper published in the *Customs Gazette* in 1883. Here he emphasized the vital importance of early diagnosis, anticipatory, if possible, of accumulation of pus, believing that the termination of hepatitis in a definite abscess might thus be prevented, or that if an abscess had formed it could be cured. He did not make light of the difficulty either of detecting the exact site of impending (or commencing) suppuration by surface examination, or of striking it by aspiration, having himself had to deal with abscesses so secluded or so deep as to evade several assays of the needle, or else to require the full push home of a needle nearly four inches long, to reach them. Dissatisfied with the treatment of such deep-seated abscesses by repeated aspiration he devised an apparatus of his own for draining them. This apparatus, by means of which he was able, by a bloodless operation, to reach and provide safe drainage for the deepest and most dependent abscess, consists essentially of a trocar and cannula and a long stylet, the stylet having an eye to take a string at its near end and at its far end a cap to which a perforated drainage-tube is attached; so that when the cannula and stylet are withdrawn the drainage-tube remains. In its original form the apparatus was made to Manson's specification by a Chinese mechanic; in a later and more elaborate form it was described in the *British Medical Journal* for January, 1892 (pp. 162–67). It may be mentioned that "Manson's trocar" is still used in places where the surgeon cannot depend on skilled assistance; and, incidentally, that in a modified form it is in current usage for the surgical relief of the distended bladder.

In connexion with the early diagnosis of liver abscess,

we may here mention two rules that Manson formulated for the benefit of his successors in China, as a result of his own experience of two not uncommon and sometimes disconcerting vicissitudes of practice among Europeans in Amoy : " Given a non-febrile case in which persistent symptoms—no matter how trifling—point to disease in the chest (especially if there is laryngeal irritation) and the cause for which cannot readily be made out, suspect aneurysm of the aorta : given a febrile case of some standing in which symptoms—no matter how trifling—point to disease in the abdomen, the precise nature of which cannot be diagnosed, suspect abscess of the liver, and carefully and repeatedly examine for it."

As an illustration of Manson's readiness to make good use of an opportunity, it may be mentioned that, having once to deal with an exceptionally complete biliary fistula, following an operation for liver abscess, he devised a plan of continuous drainage, which, while benefiting the patient, enabled the doctor to collect and measure the biliary secretion continuously for a month. Concurrently a record of the urine was kept for the same term. In this case, dealing with a man of about 100 lb. weight, eating heartily of ordinary food, and not taking exercise, Manson calculated the average amount of bile secreted in twenty-four hours to be over 28 ounces, the secretion being very irregular, having no apparent relation to the hours of eating, and being to some extent vicarious of the urinary secretion. This is probably one of the earliest experiments on record to determine the amount of the biliary secretion in man.

If his notebooks reveal Manson as an inventive surgeon, bold, but very careful—as is shown by his refusal to touch eye surgery until he had seen something of it in a special London hospital—they also disclose

in him a remarkable insight as a physician, with an unusual zeal and ingenuity in seeking for the causation of the diseases he had to treat.

Leprosy was extremely common in Amoy, Manson's systematic inquiry in the city and the country round about leading him to estimate the number of lepers as near 1 in 450 of the population. He realized the magnitude of the evil, and came near to being dispirited by the difficulty of doing anything for it. "When the time comes," he wrote, "for sanitary science to have a place in the government of China, this problem of leprosy will be amongst the first to demand attention. It is a most unfortunate circumstance that Western medical science is not in a position to guide or advise —at least with confidence—on this subject. The important question of its communicability is not yet settled, except in an official way by the Royal College of Physicians. This official settlement is in the negative. But unofficial opinion now seems to be gradually veering towards a settlement in the affirmative, and indeed, in the face of certain well-known facts in the history of the disease, it is difficult to understand how its communicability can be denied. I can only explain the denial by the absolute ignorance which prevailed as to the steps of the process and the *vera causa* of the disease."

In February, 1879, being then in possession of a good microscope and much disposed, as well as well qualified, by his successful researches into the causation of filariasis, to search for a causal parasite, he began looking for one in "leper-juice," or lymph from leprotic nodules. In it he records having seen minute rod-like bodies which must have been bacilli of the same kind as the leprosy bacilli discovered in the same year by Hansen (*see* Plate IX, Fig. 2, facing p. 114). He also

records an ingenious and original device by which he attempted to cultivate these bodies—inserting capillary tubes, filled with leper-juice, in a hen's egg and using the hen as a natural incubator for the subservient egg. When we reflect that Pasteur's germ theory of disease was communicated to the Academy of France only in 1878, we can hardly wonder that Manson did not realize the significance of the rod-like bodies that he saw and "cultivated." In a paper published in 1882 when, in the craze that naturally followed the propagation of Pasteur's wonderful discoveries, men believed they had found bacteria even in scarlatina, measles, smallpox and malaria, he made a reference to these experiments of his with leper-juice—a reference which, though it has proved unfortunate, shows what was at the time a highly commendable caution. "Three years ago," he wrote, "by cultivating the juices expressed from leper tubercles, I obtained in great profusion a bacterium resembling that figured in Hansen's paper. We should be rash to conclude that this bacterium has anything to do with the causation of leprosy, and observers who hurry to similar conclusions should pause. The present is the age of bacteria, and as they are searched for everywhere and in nearly every disease, they are found everywhere and in every disease." In this paper also he expressed astonishment that Britain, with her vast colonial empire, should be so indifferent to the problem of leprosy: "England has been familiar for centuries with the scourge, but, beyond calling for reports and establishing one or two leper asylums, and other half measures, has done little."

The effects of leprosy, as of elephantiasis, upon the skin naturally attracted attention to other skin diseases, and in 1878 Manson took up the study of an intractable

kind of ringworm of the body, common in Amoy. At that time it was the current teaching of British dermatologists that ringworm of the head and ringworm of the body were identical; Manson's investigations showed quite conclusively that they differed both in causation and in clinical features, the particular ringworm of the body—which is now known to have an extensive distribution in the tropics—being caused by a particular species of fungus named by him *Tinea imbricata.* He isolated the new species, described and figured its conidia and mycelium, and accurately defined its effects upon the skin; and finally he proved his argument by inoculating the arms of two Chinese assistants with scales first of *Tinea circinata* and subsequently with *Tinea imbricata,* and reproducing the typical clinical lesions of each disease one after the other. The incubation period in both was found to be about nine days. An additional reason for separating *Tinea imbricata* from other skin diseases, and placing the fungus in a distinct species, was supplied by the peculiarity of its geographical distribution. The disease he believed to originate in the Straits of Malacca and the islands of the Malay Archipelago, and rightly considered to be identical with the " Pita, or Tokelau itch," of Samoa, of which he had also read.

Manson first began the study of blood in malaria in 1878, with an ingenious experiment which, although it led to nothing, those who remember the remarks of Darwin upon the value of what that great man termed " fool experiments " will not think of dismissing as crude or insignificant. It shows, at least, his love of experiment. The aim of Manson's experiment in this case was to discover whether there was anything in malaria blood that caused its temperature to rise in fever.

Having in his hospital a patient suffering from quartan ague, he took 2½ ounces of blood from him at the beginning of an ague-fit and compared it, for temperature, with the same amount of blood taken from three fowls. The two samples of blood were diluted to keep them from clotting, were placed together in a bath maintained at a temperature of 100° F., and were tested for temperature every ten minutes. Neither the fowls' nor the patient's blood rose above the temperature of the medium; though the patient's body-temperature rose to 105° F. In 1881, again, having heard of the *Bacillus malariæ* of Klebs and Tommasi-Crudeli, but not of the *Plasmodium* discovered by Laveran, he made a prolonged but vain search for the supposed bacillus in a case of simple tertian ague. It is tempting to speculate on what might have happened if, with his *Filaria* experience fresh in him, this case had chanced to be malignant tertian with its spectacular crescent-shape gametocytes.

These are the only notes on malaria to be found in the Amoy journal; but the correctness of Manson's attitude towards theories of the nature and causation of malarial fevers is shown in his notes on an ambiguous kind of fever that occurred, in 1880, on the island of Koolongsu, among the European residents as well as among the Chinese. It began in a group of Chinese houses on the margin of a string of paddy fields, and appeared to be conveyed by milk. The temperature charts greatly resemble those of paratyphoid B, and it was probably with this fever that Manson had to deal; that the disease was genuine typhoid was by no means certain; in some cases the symptoms supposed to be characteristic of this disease were present, in others, again, beyond the facts that the fever was continued and uncontrolled by quinine, there was no evidence of

its typhoid nature. An ingenious explanation of such anomalous fevers had been invented, but in place of a candid admission of ignorance it had been affirmed that they were a combination of ordinary typhoid and intermittent fever, and the name "typho-malarial" had been applied to them. Manson, however, did not think that there was satisfactory evidence for the existence of such things as hybrid fevers. "The truth is," he says, "we are merely entirely ignorant of a number of specific fevers which from time to time affect the inhabitants of foreign countries."

Another disease, besides this so-called typho-malaria, which baffled Manson was beriberi. There was a large camp of soldiers in the vicinity of Amoy, and Manson remarked year by year that the number of cases of a disease which he believed to be beriberi increased amongst them. Ignorance of the Honan dialect, and the absence of interpreters, hindered him from investigating the subject thoroughly. The principal features of the disease he described as ataxia of the lower extremities, absence of the patellar tendon phenomenon and of foot clonus; presence of all skin reflexes, and extreme tenderness of the muscles of the legs and thighs, especially in the calf, to pressure. The first case he saw he put down as locomotor ataxia, and indeed the resemblance was very remarkable, but as they recovered, it was evident that the diagnosis was not correct. When we consider how long the pathology of all food-deficiency diseases has had to wait on the progress of other branches of science, and, furthermore, that there are still some distinguished authorities who are not convinced that beriberi, though a "food disease," is wholly a deficiency disease, it is no wonder that it remained an enigma to Manson.

Still another puzzling disease was sprue, of which he saw a good many cases, especially among old European residents who had suffered from the insidious "effects of the climate," to whom he indulgently defined the disease as the past tense of the verb "to spree." His Notes on Sprue, published in the *Customs Gazette*, in 1880, are generally regarded as the first accurate description of the disease. There he explained its insidious and chronic nature and its restriction to European residents of the tropics; gave reasons for his opinion that the inflammation of the mucous membrane of the alimentary tract, to which the principal symptoms are referable, may begin in the bowels, or in the stomach, or in the mouth; noted the wasting, out of all proportion to the diarrhœa, and the general atrophy of the tissues, including the liver; and pronounced the prognosis to be grave unless the sufferer be quickly removed from a tropical climate.

In dealing with epidemics—as in snipe-shooting—Manson showed abilities not always associated with the studious reflective habit. His worst experience was in the Amoy cholera epidemic of 1877, when it was computed that 2 per cent. of a population of 80,000 perished of the disease, "the dead and dying lying huddled together in the corners of the streets," and there were also some tragic deaths among the European residents, both ashore and afloat. The Chinese remedies included alum, stimulants, hot applications, shampooing, and "pinching," this last method of treatment being carried out by "pinchers." These men—we desire to be understood in a literal sense and not as indulging in an ill-timed levity—applied a subtle form of stimulation to the collapsed patient by means of "cash," with which they pinched him all over. Any coolie skilled in

"pinching" and possessed of the necessary fortitude could take thirty dollars a month in this vocation. One learned Chinaman declared the dust from the hinges of the doors to be, literally and metaphorically, a priceless remedy. The prompt action that Manson took in dealing with this epidemic was to post notices throughout the town stating that medicines potent to check the premonitory symptoms of the disease would be distributed, at the dispensary, free of charge, to all comers. The number of applicants was enormous. Mandarins and other charitable wealthy persons then followed his example, and not only distributed medicines but also organized the customary devil-scaring ceremonies for the comfort of the people. Finally, when the epidemic was nearly spent the Provincial Government in Foochow sent two native doctors and a joss to grapple with it. The joss was received with all the noisy honours of guns and crackers; the native doctors were installed in the house of a local Crœsus; and Manson was invited to join the concert. He accepted the invitation, but as the epidemic had all but died down, there was little to be learnt about the Chinese official methods of contending with cholera.

It was during this fearful outbreak of cholera that Dr. Guppy, just arrived in Amoy in medical charge of H.M.S. *Hornet*, made Manson's acquaintance. He came ashore on purpose to get Manson's advice. "What shall I do?" said Guppy. "I expect you have got it on board now," said Manson; "write a letter, On Service, to your captain, and advise him to leave at once."

Smallpox, though there is no record of any epidemic outburst, was always prevalent, and Manson considered it to be the principal cause of mortality in Amoy, the

population being unfamiliar with vaccination and the death-rate from the unmodified disease about 33 per cent. Manson did not himself introduce vaccination, but he did much to make it popular, so that in 1877 he records that the Chinese "now readily submitted their children to be vaccinated, and those who were able very willingly paid a fee ranging from 100 cash to several dollars." A student trained by him—one of the class of hospital assistants which afterwards evoked the clamour of the rigorous missionary—was appointed public vaccinator in the neighbouring city of Chang Chow.

We may conclude this chapter with an amusing item from Manson's diary, showing how a Chinese doctor's fine theories were upset by what Huxley once called "a nasty little fact." In 1872 Amoy suffered from a violent epidemic of dengue fever, introduced from the Straits Settlements by repatriated Chinese emigrants. Beginning early in August, it spread like a jungle fire, attacking almost everybody, until by the end of September about 95 per cent. of the Chinese population was down, and a good many European residents also. To the Chinese doctors it was really a new disease, but, since they are reluctant to admit that there is anything new under the sun, they called it *tchoot-pia* (measles) and *hong-pia* (wind measles). One of the most eminent of the native faculty, an undisputed authority on eruptive fevers, explained to Manson that everybody is liable to at least two attacks of measles in the course of his life, once before having the smallpox and once afterwards, and that this epidemic, supposed to be something novel, was merely a second (or perhaps third) round of measles—all in proper course. However, in due time he was smitten by the disease himself,

and after a vivid personal experience of the "break-bone pains" he had to change his opinion.

Whooping-cough and smallpox were also very common in this year, and a glanders-like disease of horses also engaged Manson's attention at this time.

So far we have endeavoured to follow Manson's footsteps as a surgeon and physician patiently making way over an immemorial morass of incredulity and distrust, until at last we find him admitted to a considerable obstetric practice among the Chinese—in itself merely an episode, but in its significance one of the most signal proofs of confidence that a European doctor can receive in the East. We have next to follow him cautiously and perseveringly opening out his way, with the same remarkable ability in recognizing opportunities, into a spacious field of scientific research.

CHAPTER IV

At Amoy : The Story of Filaria and Filariasis

In describing Manson's pursuits as a surgeon and physician we have subordinated, without entirely disregarding, the chronology of his diary to a desire to give harmony and individual continuity to the various threads running through it, with varied breaks of association in a course of thirteen years (1871 to 1883). In following now his concurrent paths in pathology and biology we shall observe the same convenient method; but as an interlude, which also embodies the necessary prelude to the present quest, we must go back to the year 1875 and leave Amoy altogether.

The year 1875 was spent entirely in England and at home in Scotland. Manson sailed from Amoy towards the end of 1874, full of plans, as he told us long afterwards, for learning all that he supposed to be well known in London of the causation of the diseases he had become so familiar with in his hospital practice. He did not leave England until the very end of the following year; and here we find a happy moment to mention that on the day he left England to return, via Italy, to China, he became a married man. The lady of his thoughts was Miss Henrietta Isabella Thurburn, second daughter of Captain J. P. Thurburn, R.N., then in command of the training-ship *Arethusa*. Miss Thurburn, who was much his junior, was an allowed beauty;

he had met her in June at a dinner party at her uncle's house in London, and they became engaged very soon afterwards. After the engagement he took her to Scotland to introduce her to his numerous relatives, and on 21 December, 1875, their marriage was solemnized at St. George's Church, Campden Hill. Manson was then 31 years of age, though the fine portrait taken at the time (facing p. 26) gives him, without detracting from his look of vigour and resolution, the more serious expression of an older and riper man. Anterior to this felicitous interlude Manson had worked in London, among other doings attending the Eye Hospital at Moorfields, where he made acquaintance with Nettleship. But in other directions, his somewhat pathetic belief in the existence in London of a fountain of living knowledge on the subject of tropical pathology was quite upset. He did not know London: he does not seem to have had introductions to any scientific circles: the only source of authentic instruction of which he in his isolation could think was, of all places for an enthusiastic pathologist, the library of the British Museum in Bloomsbury, and there he studied with his usual application. Setting himself to find out all that was known about chyluria and elephantiasis, he learnt of a microscopic nematode worm, a *Filaria sanguinis hominis*, that had been found in Calcutta by Timothy Lewis [1] both in chylous urine and in the blood of a patient who had at one time suffered from chyluria. This piece of information was to him the grain of mustard seed, which

[1] Timothy R. Lewis, F.R.S., a greatly distinguished officer of the Army Medical Service (as it was then styled), born 1841, died untimely in 1886. His is the name so familiarly associated in the annals of Tropical Pathology in a noble parity with that of D. D. Cunningham, F.R.S., of the Indian Medical Service.

he took and sowed in his field when he got back to Amoy, where it grew into one of the greatest amongst herbs for the service of men whose pabulum is Tropical Medicine.

Before beginning the long story of Manson's labours in this field it is necessary to give a brief account of the history of the filaria from the time of its discovery to the time when the results of those labours began to be known.[1] It is necessary to do so because, although all the facts have been common knowledge for more than forty-five years and are to be read in every textbook of Parasitology, Manson's numerous contributions to them, notwithstanding that the seal was set upon their originality and high significance at the International Medical Congress of London of 1913, have been belittled and decried and even denied by certain recent non-medical writers.[2]

The *Filaria sanguinis hominis*, found in Calcutta by Lewis in 1870 and 1872, had actually been discovered in 1863 in Havana by the French surgeon Demarquay in a case of chylous hydrocele, and was again twice "discovered"—in 1868 and in 1869—in cases of chyluria elsewhere. Lewis, therefore, though not the first to find the parasites in chylous urine, was the first to discover them in the blood as well, and the first to understand that they must have some definite pathological significance. He also realized that they were immature forms, but though for five years he missed no opportunity in Calcutta of a search for the parent

[1] *See* Cobbold in *Journal of the Linnean Society*, Zoology, vol. xiv., 1879, pp. 356–70. A grievous pedagogic account, but emphasizing Manson's "remarkable discovery of the intermediate host" of the *Filaria sanguinis hominis*.

[2] *See* Ray Lankester, in *Nature*, vol. cix., 1922, pp. 549, 812.

worms, it was not until August, 1877, that he made a most conclusive discovery of both sexes of the adults in a nævoid elephantiasis scrotum removed from a patient whose blood contained the embryos also. Meantime in 1876 Dr. Joseph Bancroft,[1] in Brisbane, discovered filariæ in the blood of persons passing chylous urine, and sent specimens of them to Roberts, the famous renal specialist of Manchester, who identified them with the worms described by Lewis in 1872 and then passed them on to Cobbold, the leading British helminthologist. Cobbold, who recognized them as embryos, communicated the fact to Bancroft and set him on the look-out for the adult forms. These were discovered by Bancroft, in December, 1876, in a lymphatic abscess, and were sent to Cobbold, who named them *Filaria bancrofti* and published the news of their discovery, in July, 1877—just three and a half weeks before their discovery in Calcutta by Lewis. (Cobbold's actual description of the adult *Filaria bancrofti* was, however, not published until the week after Lewis's description.)

These are the principal relevant facts, in historical sequence, regarding the discovery of *Filaria bancrofti* and its embryo *Filaria sanguinis hominis*; though they show that Lewis as a helminthologist was anticipated at both ends of the story, they make it clear enough that it was he as a pathologist who apprehended and first endeavoured to explain the pathological meaning of the parasite. And it was Lewis's work that made the momentous impression on Manson.

The facts discovered, however, did not disclose the whole life-history of the parasite, and left the pathological field uncertain and undefined. They gave no clue of

[1] Not to be confused with his son, Thomas Bancroft, who wrote about the transmission of filaria twenty-three years later.

any stages of development from embryo to adult; they did not indicate the precise situation of the adult in the human host; they threw no certain light upon the nature and circumstances of the pathogenous action of the parasite,[1] and how far such action might be due to the adult and how far to the embryos; and they left the manner in which the human host becomes contaminated quite unsuspected. It was to all these questions that Manson patiently and sagaciously devoted his attention during the years between 1875 and 1883. And when we take into account his circumstances—the medical and surgical charge of a large native hospital, the responsibilities of a Port Health Officer, the cares of a large private practice, the round year near sea-level only about a degree outside the tropic—when we keep all this in mind as we follow his independent course as a scientific investigator, we shall realize what a great heart, literally as well as metaphorically, the man must have had, quite apart from the big mind revealed in the results.

Even in 1875, before he got back to Amoy, he had constructed a working hypothesis as a guide to the location of the adult parasite and to the explanation of its pathological influence. This hypothesis [2] is a beautiful instance of that almost prophetic insight which is only found in clear thinkers who are also good workers —an inherent quality of the mind, not an acquirement. Manson argued from what was known of *Filaria immitis*, a common blood parasite of the dog, that the adult

[1] Even as late as 1885, Roberts, in his classical work on Urinary Diseases, quoted, without disapproval, the view which ascribed the pathogeny to blocking, and consequent irritation and rupture, of the capillaries by aggregations of the embryos.

[2] *Customs Gazette*, xxvii., No. 10, Part 6, 1875, pp. 1–14.

Filaria bancrofti might be attached to the wall or neighbourhood of an artery, a vein, or a lymphatic vessel; but since he had already (in 1872) concluded from his surgical observations [1] that lymph-scrotum was "a sort of lymph dropsy" due to obstruction of neighbouring lymphatic glands, which might also be the origination of scrotal elephantiasis, he now decided that the most probable site would be "in the lymphatics,[2] the receptaculum chyli, or thoracic duct, or some blood-vessel in the neighbourhood of these." As a result of lymphatic obstruction and accumulation of lymph behind it there might, he argued, result rupture of the lymphatics of the bladder, shown as chyluria; or of the scrotum, shown in lymph-scrotum; or lymph stasis resulting in tissue proliferation, shown in elephantiasis. In short, we have here a conception of the pathology of chylous fluxes and tumours as a mechanical business, and, as such, neither inevitably, nor even perhaps exclusively, the result of filarial infection; a conception which is generally current at the present day. The predicted location of the adult filariæ in the lymphatics was verified next year, when Bancroft found them in a lymphatic abscess, and again when Lewis found them in an elephantoid scrotum, but not by Manson himself until at the end of five years' search he found them in a dilated lymphatic in a lymph-scrotum. The conclusive verification of the assumed lymphatic obstruction took him almost as long a search to complete.

Early in 1876 Manson was back in Amoy, having now a much more powerful microscope. With it he went

[1] *Customs Gazette*, xiii., Part 6, 1872, pp. 26–33.

[2] The adults of both sexes are now known to live together in some part of the lymphatic system—in the lymphatic vessels or in the lymphatic glands, or in lymphatic varices.

about to find how far the native population of Amoy might be infected with *Filaria bancrofti* (to give it its later-bestowed name) and to familiarize himself with the behaviour of the parasite. At the end of the year he had found out, as he stated in the *Customs Gazette*, that the embryos were present in the blood of 15 out of 190 Chinamen examined, that they were sometimes present in the blood of men of *unblemished appearance who seemed to be in sound health*, although they were generally found in men having either some sign of elephantiasis, or some indication of ill-health shown by recurring attacks of fever often concurrent with a general anasarca not due to disease of heart or kidney.

During 1877 he continued his observations, and in September he published "Further Observations on *Filaria sanguinis hominis*."[1] Up to that time he had examined 670 persons, and, allowing for the temporary absence of the parasite from the blood-stream, which—the first indications of "periodicity"—he found was not infrequent, he computed that one in every 10·8 persons in Amoy was the subject of this infection; that the liability to filariasis increased from youth to old age, gradually rising from 1 in 17·5 to 1 in 3; and that elephantiasis and cognate disease were associated more frequently with this parasite than with any other morbific condition. Making every allowance for the imperfection of his statistics, these facts convinced him of the causal connexion between the filaria and elephantoid disease. "It may be asked," he says, "if the filaria is the cause of elephantoid disease, why are the embryos not invariably found in the blood; and why, when the embryos are found in the blood, are they not invariably

[1] Customs Reports, Special Series, No. 2, 14th Issue, 1877, pp. 1–26.

associated with elephantoid disease?" The second question is easily answered. The disease in the host is not a *necessary* consequence of the existence of filariæ in or near the lymphatics. As the *Filaria immitis* seldom inconveniences the dog in whose blood it lives and breeds, so the *Filaria sanguinis hominis* may live and breed in or near man's lymphatics without inducing disease. Here, as again and again, he emphasizes the opinion, based on his own observation, that the disease is only a *frequent accident* in the history of the parasite. The same questioning of Manson's parasite theory of elephantiasis was made by specialists in London, two years afterwards, when his work began to be known in England, and it was answered by him in the following homely but convincing speech:

"To say that, because there are many with filariæ in their blood without having elephantiasis, therefore filariæ are not the cause of elephantiasis, is not correct reasoning. We do not find every one who has atheroma has an aneurysm as well, or that every one who gets rheumatic fever gets heart disease, or that every one who drinks spirits has a cirrhosis of the liver; these secondary effects of recognized causes are not necessarily *always* consequences—the cause may exist, though, from some circumstance being absent, the recognized consequence may not follow; the diseases are accidents—characteristic accidents. And so with chyluria, lymph-scrotum, varicose glands and elephantiasis of the scrotum—they are the result of the presence of the parasite, but by no means the *necessary* consequence; there was one feature they all had in common—the enlargement of the groin glands."

In his 1877 report Manson also noticed the very significant frequency of hydrocele in Chinamen infected with the parasite.

All the time evidence went on accumulating which, while it also defined the pathogenous import of *Filaria bancrofti*, explained the pathogenous effects as being

mainly of a mechanical nature and originating in obstruction of the lymphatic glands. While continuing to observe cases—simple, or concomitant, or alternating—of chyluria and lymph-scrotum concurrent with the presence of the embryos in the blood, Manson also reports cases—all of them indicating a common line of obstruction—of lymph-scrotum passing into, or coincident with, or sequent to removal of, elephantoid tumours of scrotum or leg, and now and again cases where a careful search seemed to throw light upon the nature of the obstruction. These latter cases (like the former) were recorded in the Customs Reports and were rescued from oblivion in a little book brought out by Manson some years afterwards (1883) and published by H. K. Lewis under the title "The Filaria Sanguinis Hominis and Certain New Forms of Parasitic Disease in India, China and Warm Countries," so that no more need now be said about them than is necessary for explaining Manson's sage grasp of them.

From his own, apart from others' observations, Manson knew that adult filariæ lived—wherever else they might live in that system—in a lymphatic channel in the interior of the body. There the female, like other species of the genus, being viviparous normally, would discharge her myriad embryos into the lymph-stream; and the embryos, being no broader than a lymphocyte, would easily traverse the lymphatic glands on their final road to the blood-stream. Thus would be justified the observation that embryos could be found in the blood of men who seemed to be in perfectly sound health and without blemish. But in certain cases Manson met with *eggs* with the embryo coiled up inside them: in two instances the eggs were in fluid from a lymph-scrotum, in one case they were in lymph taken from an

indurated inguinal gland. These eggs must have come untimely from a mother, and in two cases at any rate she must have been on the far side of a lymphatic gland. The eggs, having a minor diameter five times that of the embryo, may reach a lymphatic gland but cannot traverse it; they must therefore act as emboli within the gland, and—whatever may happen afterwards to exaggerate it—they must be the original cause of the lymphatic obstruction. Manson in his homely phrase assumed the premature passage of eggs to be a "miscarriage," all female flesh being liable to drop unripe fruit; but he did not rest satisfied with assumption; he convinced himself by observing the processes of parturition in the common filaria of the white-necked crow—a minute filaria that can be kept under continuous observation beneath the microscope—that miscarriage actually does occur in species of the genus *Filaria*.

Manson's contributions to the pathogeny of *Filaria bancrofti* and his explanation of the mechanism of the filariasis studied by him in Amoy have the characteristic openness, breadth, and unpretentiousness of their author. They do not claim to be the exclusive, exhaustive, imperative truth; but they do represent fine, honest, lasting work—substantial foundations, upon which a considerable variety of workers can trust. After the lapse of forty-five years it may fairly be said that if they do not contain the whole truth about filariasis, they contain nothing but truth and nothing but what is very much to the point and very generally instructive.

In 1876 and 1877 Manson had frequently noticed that in patients whose blood he knew to be infested with filariæ the embryos sometimes could not be found. Afterwards, when he had trained a couple of Chinese

assistants to carry on a systematic survey of the blood of Chinamen and appointed one of them to make examinations during the day, and the other to make them after 6 o'clock in the evening, he was struck by the fact that the day observer made very few "finds" compared with the evening observer. As a check he set them both to examine one particular patient, and still there was the same discrepancy between diurnal and vespertinal results; but he remarked, as a significant accident, that on the days when the press of hospital work was so great that no time could be found for any observations until the evening, filariæ could always be found in abundance in the patient's blood, whereas on the days when the ordinary run of hospital work permitted the observations to be done in the daytime, filariæ were few or absent. He therefore, in 1880, arranged for a series of four-hourly observations of the blood of two filaria-infected patients to be continued during ten consecutive days and nights, and thereby discovered the explanation of all the early discrepancies, which he ultimately expressed in the generalization that in filarious patients (unless they are also suffering from some concurrent derangement) the embryo filariæ disappear from the cutaneous blood about 9 or 10 o'clock in the morning and do not reappear until sunset; after their reappearance they increase in numbers until midnight, and then gradually decrease up to their disappearance again about 9 or 10 o'clock the following morning, and this with the greatest regularity, day after day.[1]

[1] It was not until near the end of April, 1897, that Manson actually discovered, at the post-mortem of a filaria patient in London who had committed suicide by taking prussic acid at half-past eight o'clock in the morning, that the embryos retire to the lungs and large arteries during daytime. (*See also* p. 191.)

The discovery of a quotidian tide in the affairs of the embryonic filariæ was published in the Customs Reports,[1] and Manson also communicated it to Cobbold. But when Cobbold made it known at a meeting of a well-known scientific club in London it was received with bushels of chaff, a Gratiano present displaying one of his few grains of wit in the question "whether the filariæ carried watches." We need not follow the subsequent history in other authors' hands of this well-established "Law of Periodicity" as it is called; how it can be inverted by reversing the customary hours of sleeping and waking, and how it does not prevail all round the tropical zone but appears to be dependent on the abundance or otherwise of a particular species of vespertinal mosquito of circumtropical range—*Culex fatigans*. What interests us here is its biological beauty, and the manner of its discovery; since it illustrates one of Manson's pungent maxims as a lecturer in after years to medical graduates—that it is the discrepancy that teaches, if you would learn.

Though he recognized that this periodicity had a pretty teleological explanation in the habits of the mosquito *Culex fatigans*, Manson made a deeper search for a critical reason, particularly because, in the collateral studies of the blood of animals that he had been carrying on all the while, he had noticed a periodicity of a sort in the common flagellate of the blood of the rat, *Trypanosoma lewisi*. To begin with he turned to the *Filaria immitis* of the dog, and found that it too had a quotidian period in the cutaneous blood, but that this was shown in an abatement of numbers, not a complete disappearance, of embryo filariæ during the daytime, when they were massed in the capillaries of the lungs. How they

[1] Customs Reports, Medical, 23rd Issue, pp. 1–16.

maintained themselves there was not altogether perplexing, since he had observed that the embryo filariæ of the white mynah (a common bird in Amoy) were able to anchor themselves to the wall of a blood-vessel. The examinations of the dog had shown that the embryos of *Filaria immitis* did not die off during the daytime; and continuous examinations of lymph from an oozing lymph-scrotum and of chylous urine showed by the persistence of embryos in those fluids during the daytime that the process of parturition in *F. bancrofti*, once begun, was not suspended in the daytime; the periodicity therefore must depend on circumstances existent in the blood. Looking for possible circumstances in meteorological fluctuations, Manson observed none that bore upon the point, since the day was not always warmer than the night, nor the barometric pressure always lower during the afternoon, and by continuous observations, for several days, of a filaria-infected patient kept in a dark room, he found that the normal periodicity was not altered by absence of sunlight. Except for the teleological and barren verbal explanations, the "cause" of filarial periodicity is still unknown, and it is left open to us to assume that the relaxation of peripheral blood-pressure, which normally occurs in man during the hours of sleep (whether at night or in daytime) must widen the excursions of the filariæ, and this assumption is in accord with Manson's final opinion that the explanation must be left to the physiologist.

As an unconscious self-revelation of simplicity and modesty the following unpretentious letter, in the course of which Manson makes known his discovery of this filarial tide, eclipses all the art of the biographer.

STORY OF FILARIA AND FILARIASIS

MANSON TO T. SPENCER COBBOLD

Amoy, 20th June, 1879.

I will forward you by this mail filaria-impregnated mosquitoes. They are preserved in glycerine and were fed on the blood of the man whose case I append. . . . I hope you will pardon this delay in sending the mosquitoes; being in general practice here, the many interruptions this entails make work of this sort exceedingly difficult to carry out quickly. I read in the *Lancet* lately an account of the discussion on a lymph-scrotum sent from India, and felt disappointed that the filaria was not found. I therefore determined to send you the first scrotum I amputated in which I had unquestionable evidence of filariæ. The scrotum I send is the result, and to complete the case I send particulars of the man's history, and the result of the examination of his blood before and after the operation. The case is of much interest as it exhibits, first, the transition from lymph-scrotum to elephantiasis; second, it demonstrates unmistakably that the parent worm is not necessarily present in the affected tissues themselves, though probably in proximity. I had hoped you might find the *Filaria bancrofti* in the scrotum, but the embryos persisting in the blood weeks after the operation show that this is unlikely. Third, it illustrates well a new fact in the history of the filaria. The young escape into the circulation at regular intervals of twenty-four hours, the discharge commencing soon after sunset and continuing till near midnight, from which time till the following noon their numbers gradually decrease. By 2 or 4 o'clock till 6 they are nearly completely absent. This is a striking and most suggestive fact, and in connexion with it one might be tempted to speculate on the causes of the periodicity of malarial fevers. It is marvellous how nature has adapted the habits of the filariæ to those of the mosquito. The embryos are in the blood just at the time the mosquito selects for feeding. Another fact in adaptation you might like to know. The long lash on the tail of the embryo has a meaning in relation to its future life. I think so from the following experiment. Drop a few fibres of cotton-wool into the fluid of a "milk" (filarial) hydrocele. They will subside to the bottom of the vessel very gradually; leave them there for a few minutes, and then place them under the microscope. You will find them beset by thousands of embryos in rows and clusters, each embryo attached by its tail-lash as one can attach a whip to a stick by striking it sharply

with the lash. When the mosquito penetrates a blood-vessel the passing embryos, lashing about as is their habit, entangle themselves on the proboscis and get sucked up. Hence the enormous numbers of embryos in the mosquito's stomach and the selecting faculty of that insect.

As far as I have examined, I find that in other cases the embryos observe the same periodicity. I have a gardener who comes from a filarious district. I knew his blood sometimes contained worms. It was examined on three occasions during the afternoon without finding embryos. Yesterday evening it was examined about 9 o'clock and was swarming with filariæ.

If you think these facts of sufficient importance to make public, I would be much obliged by your doing so in any way you think best. They have not left my notebook before. I am gradually accumulating evidence to prove—I hope to the satisfaction of such cautious sceptics as Dr. Tilbury Fox and others—that elephantiasis is a parasitic disease. I have got some strange results from tapping enlarged groin glands with the subcutaneous syringe, but until the chain is completed either by myself or others, I will keep silent on this point.

It seems to me that Lewis by his great discovery has opened a new field in tropical pathology. The interest and importance of *F. bancrofti* and *Filaria sanguinis hominis* is by no means exhausted yet. I hear Lewis is in England, but when he returns to India I hope he will take up the subject again.

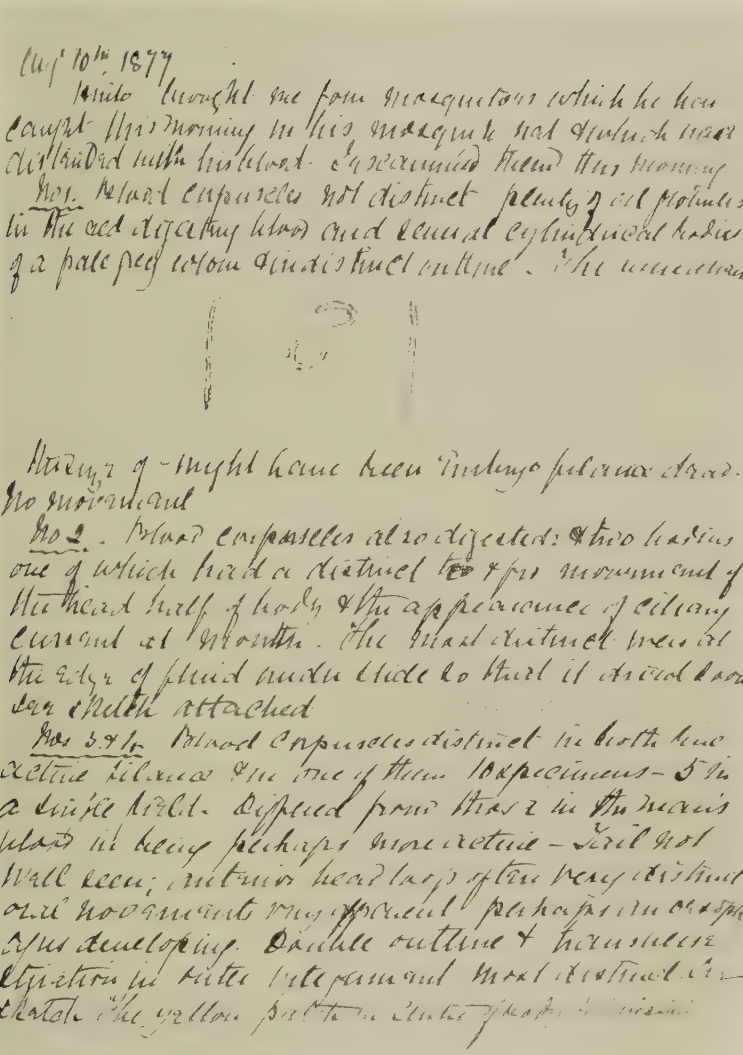
Men, like myself, in general practice, are but poor and very slow investigators, crippled as we are with the necessity of making our daily bread.—Believe me yours faithfully, PATRICK MANSON.

In the summer of 1877 Manson began to reflect upon the destiny of the embryo filariæ: what becomes of them all? How and where do they undergo the development that permits them and fits them to take up with another host? He calculated that in the vessels of certain patients there might exist at any moment more than two million embryos. Obviously they all cannot develop in their present host without killing him and thereby frustrating their own persistence as a species; therefore, like the eggs and embryos of other parasites

which live in the interior of their hosts, they must escape from their present host and then, either in a free environment, or (more probably) in the tissues of some intermediate host or hosts, grow into larval or pre-adult forms which, either in dirty water, or encysted in the flesh of the intermediate host, must be ingested by their final host, man. Undoubtedly the embryos are voided occasionally, particularly in cases of chyluria; but as their usual range is the blood, Manson concluded that they must escape from the blood by the intermediation of some common bloodsucking insect, and that that insect will be the intermediate host required. Of common bloodsucking insects there are fleas, bugs, lice, mosquitoes and sandflies. Manson excluded the first three because, as cosmopolitan insects, they could spread the filaria all over the world, whereas filariasis was prevalent only in warm countries; he selected mosquitoes because, as he then thought, their geographical range corresponded with that of the disease. (Our ignorance, like our indiscretion, sometimes serves us well; in 1877 Manson, like most of his contemporaries, did not know that the mosquito, far from being restricted to warm countries, utters forth its dolorous voice from pole to pole almost.) With female mosquitoes, therefore, he made the experiments referred to in the China Customs Medical Reports for September, 1877, and communicated, through Cobbold, to the Linnean Society in 1878. In this communication he describes how he fed mosquitoes upon a man whose blood swarmed with embryo filariæ; how, subsequently, he found embryos in the stomach of the mosquitoes in such numbers (a hundred-and-twenty in one particular instance) as to make him think that the insect must in some mechanical way "select" them from the blood;

how most of the embryos suffered digestion, and how those that escaped digestion proceeded to develop in the mosquito; how he followed the development up to the fourth or fifth day, when many of the mosquitoes died; how a few mosquitoes lived on until the fifth or sixth day, and how in four of these he found what he believed to be the final larval stage of the filaria. He describes this larva, its comparatively large size, its structure in part, its "boring" or "piercing" papillæ at one end, "probably the boring apparatus to be used in penetrating the tissues of man and escaping from the mosquito," its activity, and the fact that "it is by no means inconvenienced by the water in which it has just been immersed." Finally, he states his opinion that it is undoubtedly the larva of *Filaria bancrofti*, "equipped for independent life and ready to quit its nurse the mosquito," and predicts that its next course will be to free itself from the mosquito when the insect dies (as he then supposed it did) in the water where it has deposited its eggs, and then, coming into "contact with the tissues of man" and "either piercing the integuments, or . . . being swallowed," will "work its way through the alimentary canal to its final resting-place." The following quotations from a speech of his reported in the *Daily Telegraph* reveal the quiet fervour with which he followed the verification of his hypothesis:

"I shall not easily forget the first mosquito I dissected. I tore off its abdomen and succeeded in expressing the blood the stomach contained. Placing this under the microscope, I was gratified to find that, so far from killing the filaria, the digestive juices of the mosquito seemed to have stimulated it to fresh activity. And now I saw a curious thing. The little sac or bag enclosing the filaria, which hitherto had muzzled it and prevented it from penetrating the wall of the blood-vessels in the human body, was broken through and discarded."



Aug 10th 1877

Hinto brought me four mosquitoes which he has caught this morning in his mosquito nat & which was distended with his blood. I examined them this morning

No 1. Blood corpuscles not distinct plenty of oil globules in the red digesting blood and several cylindrical bodies of a pale grey colour & indistinct outline. The [illegible]

Stretch of - might have been Anthrax filaria dead. No movement

No 2. Blood corpuscles also digested: & two bodies one of which had a distinct [illegible] & fro movement of the head half of body & the appearance of ciliary current at mouth. The most distinct were at the edge of fluid under slide so that it dried soon see sketch attached

Nos 3 & 4. Blood corpuscles distinct in both the active filaria & in one of them 10 specimens - 5 in a single field. Differed from those in the man's blood in being perhaps more active - Tail not well seen; anterior head loop often very distinct oral movements very apparent perhaps an oesophagus developing. Double outline & transverse striation in outer integument most distinct see sketch. The yellow patch in centre of body [illegible]

PLATE VI.—Reduced facsimile of page from Manson's diary in 1877, just after he had found the filaria in the mosquito.

Manson grasped the fact that, as with a great Darwinian humility he expressed it, he "had stumbled on an important fact with a distinct bearing on human pathology."

"I followed it up as best I could with the meagre appliances at my disposal, and, after many months of work, often following up false scents, I ultimately succeeded in tracing the filaria through the stomach-wall into the abdominal cavity, and then into the thoracic muscles, of the mosquito. I ascertained that during this passage the little parasite increased enormously in size. It developed a mouth, an alimentary canal and other organs. . . . Manifestly it was on the road to a new human host."

Cobbold accepted Manson's work, and in March, 1878, wrote unreservedly of "Manson's remarkable discovery of the intermediate host." Other authorities were not enthusiastic. Lewis, as the following letter shows, at first expressed some polite doubts, although afterwards he confirmed the fact that the embryos were imbibed by the mosquito and underwent some not quite clearly determinate metamorphosis in the insect. Sonsino[1] also, in Egypt, made some confirmatory observations. Leuckart at first was an unbeliever, and seems never to have given Manson's discovery anything but a grudging and disparaging acknowledgment.

FROM T. R. LEWIS TO MANSON

Calcutta, January 14th, 1878.

MY DEAR SIR,—I have much pleasure in forwarding to you by to-day's China mail the desired papers regarding Hæmatozoa, and

[1] Prospero Sonsino (1835–1901) was a curious, wayward, peripatetic scientific worker. From 1873 to 1885 he resided in Egypt and worked under very favourable circumstances in the Khedivial laboratories. Subsequently he travelled extensively in South America and in the Far East. Of his 135 publications, 75 refer to helminthology.

in doing so, beg to return my best thanks for the very valuable report and lithographs which you were so good as to send. Allow me also to congratulate you on your extremely interesting observation regarding the embryonic nematode in the mosquito. I had frequently examined these insects, but in a cursory way, but had not observed any parasites in them at all resembling the embryo, *Filaria sanguinis hominis*, until I received your note. On receipt of this I repeated such examinations and found that several of the mosquitoes which were examined contained little nematodes resembling most perfectly those found in human blood. Whether they are actually identical or not it would, perhaps, hardly be safe to assert positively without further experience, but given slides with specimens of each kind I should be very sorry to be asked to decide which had been derived from the mosquito and which from man. With regard to the larger parasites to which you refer, you will doubtless be aware that the mosquito, like nearly all insects, occasionally harbours different kinds of parasites. It will, of course, occur to you that the due consideration of this circumstance is of prime importance in drawing any inference as to the genetic connexion between the various parasites observed. Awaiting with interest the copy of your further report on these subjects which you kindly promised, believe me yours truly, T. R. Lewis.

Stirred by this indifference to a discovery of such pathological importance, Manson in 1883 repeated and amplified the observations of 1877 and communicated the results, through Cobbold, to the Linnean Society.[1] In this publication he not only describes in full detail, step by step—and figures—the changes that take place in the course of the development of the filaria from embryo to larva within the tissues of the mosquito, but he also explains, for the benefit of other critical observers, all the details of his technique. He remarks upon the extraordinary changes of form that occur in the course of development, changes " so great that unless one had

[1] *Transactions of the Linnean Society*, Zoology, vol. ii., Part 10, April, 1884, pp. 367–88, Plate xxxix.

carefully traced the connecting links, step by step, one would positively declare them organisms of entirely different species." He explains, further, that of the four species of mosquitoes infesting Amoy the development is completed in one species only (a species recognizable from his description as *Culex fatigans*), although it can continue up to a certain point and then miscarry in one other species (recognizable as *Stegomyia*, probably *S. fasciata*). He describes the appearance and habits of the species of mosquito in which the development is completed, the temperatures at which it thrives, and the season of the year when it is most readily infected by the filaria—in these ways prophetically defining the functions of the medical entomologist. He notices, again, that although in the earlier stages of development the filaria is injured or killed in water, and therefore must be observed in a saline solution, yet in the final active stage " water appears to have a vivifying effect upon it "—a fact that confirmed him in his unfortunate conclusion that the larva was destined to reach man through that medium. Again, however, he notices how the energetic larva turns up its head as if to displace the cover-glass, and thereby displays the exceedingly minute horny papillæ which " may be the boring apparatus." " All the principal facts I have stated are true. I have verified them over and over again," are among his concluding remarks.

Although Manson rather emphasized the larval boring apparatus which he thought could be used in " piercing the integuments " of the prospective human host, he stuck to the alternative declaration that the larvæ were imbibed in drinking-water after escaping from the drowned mosquito. *Nemo mortalium omnibus horis sapit*, and instead of relying on observations of his own he

made the mistake, unusual for him, of trusting the book on natural history which informed him that adult mosquitoes were ephemeral insects, the female perishing in the water where she laid her eggs, the processes of reproduction *once* completed. He did not know—it is doubtful if anyone knew until about twenty years afterwards—that his mosquito, *Culex fatigans*,[1] can live—even in the tropical hot weather, when that insect's normal span of life is at a minimum—at least five weeks, and can produce a batch of eggs every week, and that in order to do so she must return from the nursery in the water every time and feed freely on living blood during every interval. Had he known this, he would probably have thought even more of the boring apparatus and its probable functions.

It appears to have been a very able and appreciatory, but anonymous reviewer[2] of Manson's work who, in 1883—even before the publication of Manson's second Linnean paper—first suggested that the full-grown larval filaria, as he calls it, the " parent," is " deposited by the mosquito in the act of biting." This suggestion seems to have been still-born, since it was not until sixteen years

[1] " During this time I investigated a good many other local diseases, but felt the need of books, criticism and medical companionship. For example, a regrettable mistake, the result of a want of books, was my belief that the mosquito died soon after laying her eggs. For this error I was not altogether to blame, for those books of natural history to which I had access gave little or no information about these insects, whilst one misled me by a very positive misstatement. Again, in the matter of apparatus I was handicapped. No incubator was available until I had manufactured one out of a kerosene-tin and a packing-case. Its odour still pervades my nostrils."—Quoted by L. W. Sambon, in *Journal of Tropical Medicine*, 15 June, 1922.

[2] In the *Veterinarian*, March, 1883.

afterwards, when Ross had shown how malaria parasites are injected by the mosquito in the act of biting, that it was made again by the younger Bancroft,[1] who had verified Manson's work in an Australian species of *Culex*. This time it was confirmed by G. C. Low,[2] who, working with filarious mosquitoes sent to Manson by the younger Bancroft, traced the filaria larvæ along almost the entire length of the insect's proboscis and lying in a favourable position for an attack on the skin of the insect's prospective victim. It may be added that, just before this, S. P. James, who had heard of the younger Bancroft's suggestion, and had been following the development of the filaria in two Indian species of *Anopheles*, had observed well-grown larvæ in the insect's head and partly in the proboscis.

FROM S. P. JAMES TO MANSON

Quilon, Travancore, 30.5.1900.

DEAR DR. MANSON,—In October, 1899, you kindly wrote me regarding the metamorphosis of the *Filaria nocturna* in the mosquito and advised me to continue my work in this connexion. Since then I have . . . worked out in *Anopheles rossii* and in another species of the *Anopheles* genus of mosquito the metamorphosis to its final stage. . . . The important points in my work are, I think, my finding very efficient intermediary hosts of the parasite and my discovery of the presence of filariæ in the head and partly within the proboscis of the mosquitoes—this latter point I found out at the end of April last. I am preparing slides of filariated mosquitoes and I hope soon to send them to you.—Yours faithfully, S. P. JAMES, Captain I.M.S. [3]

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[1] Thomas Bancroft, *Journal and Proceedings of the Royal Society of New South Wales*, vol. xxxii., 1899, p. 62.

[2] *British Medical Journal*, 16 June, 1900.

[3] *See also* letter dated Quilon, 30 May, 1900, *Indian Medical Gazette*, July, 1900, vol. xxxv., p. 277.

Although Manson's investigations did not show exactly how the grown filaria *larvæ* reach their final host, they did show, precisely and conclusively, how the *embryo* filariæ have to be freed from an infected human being and be specially nurtured while they pass through a larval development outside the human body before they can infect other human beings, and that common house mosquitoes are the *necessary* agents in these processes of liberation, nurture, and dissemination. Manson's investigations were thus the first convincing evidence that the vague beliefs traditional among many untutored races and countenanced from time to time by a few sagacious medical men—beliefs that various specific diseases of the blood are disseminated by blood-sucking insects—were of good pathological report and worthy of further thought. Manson's work on filariasis thus culminated in a great discovery, one of those major discoveries which, in the history of every science, are made from time to time by men who, being acute observers, are above all thinkers and make the right use of opportunities that are open to everybody—unnoticed. Discoveries of this kind, when they are made, are so obviously true that they generally clear the way for a flight of minor discoveries of the same kind. That Manson's discovery did not have this immediate effect is probably due to several causes. For one, he was an unknown man, working quietly in a far-off place from an instinctive love of the thing, as, when a boy, he dissected the cat and marvelled at its tapeworm, on the secluded roof of his father's house. At the moment he had no intimate friends in scientific circles, and when his first candle was lighted in England not the most charitable person can say that it was forthwith set in a candlestick for him. Then, again, filariasis, although

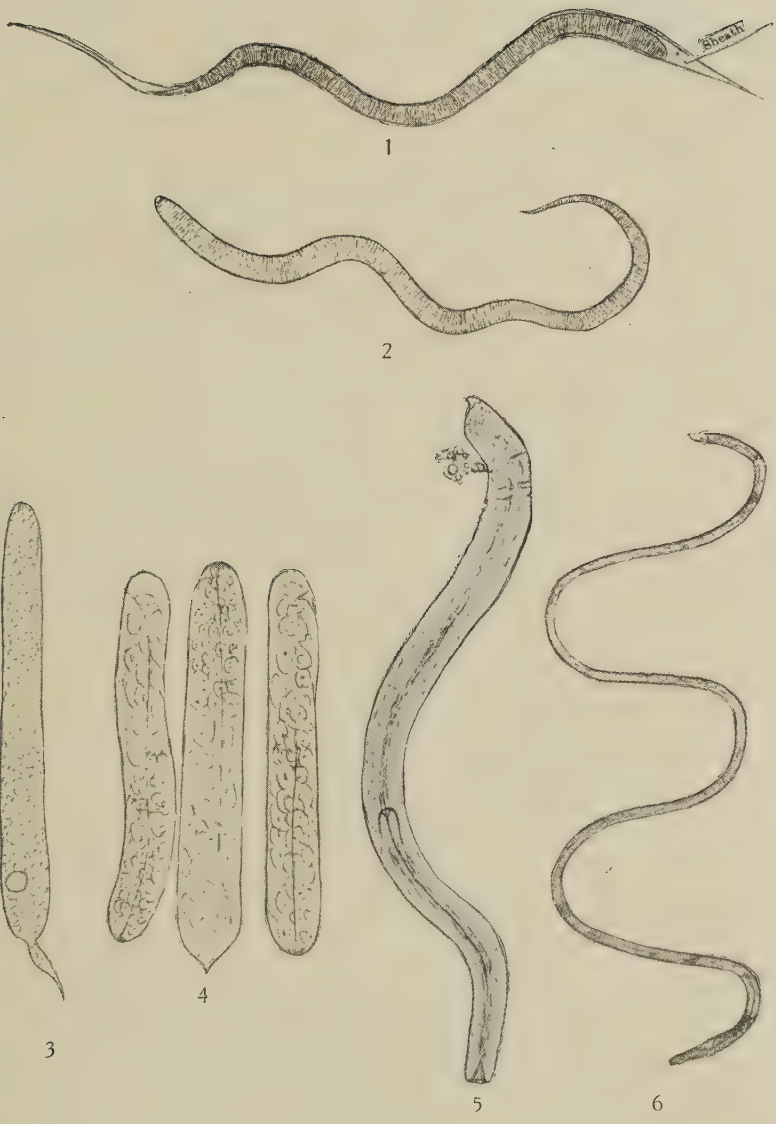

PLATE VII.—Selection from Manson's drawings of the metamorphosis of *Filaria bancrofti* in the mosquito.

1. Embryo a few hours after ingestion. 2. Embryo released from sheath. 3. About end of third day of development. 4. About fifth day. 5. About sixth day. 6. Final stage in mosquito.

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a grievous and very prevalent disease within its geographical limitations, is never a fulminating catastrophic pestilence, nor a foul disease that feeds upon the pith of industrial life; it does not affect the sensible warm motion of trade and commerce, but is merely a cold, dull, permanent obstruction to the sequestered life of the apathetic noiseless half of mankind. Nevertheless, Manson's discovery of the life-history of *Filaria bancrofti* in the mosquito intermediary host did, many years afterwards, and by Manson's own indication of its relevance, show the road that led to Ross's discovery of the life-history of the malaria parasite in the mosquito intermediary host, as Ross himself, in one of his early papers, fully acknowledged.[1]

As has been said, original discoveries in science come from time to time, as technical facilities continue to improve, from some one exceptional man of the time who makes the one right use of opportunities " by Nature left free for all men to deal withal, as very sufficient for all men's use, and large enough for all men's industry," as Francis Drake's chronicler remarked of the Ocean. Several of Manson's contemporaries made a rightful use of the common opportunities, though they did not soar as he did with certain flight into undiscovered realms. Among the distinguished contemporaries was Dr. Joseph Bancroft, whose discovery in 1877 of the adult filaria named after him has already been noticed. It would be superfluous to say more of Joseph Bancroft in this place were it not that nearly forty years after Cobbold's[2] comprehensive declaration that " to Dr. Manson must be awarded the crowning honour of

[1] Ross, R., " Malaria and Mosquitoes," *Nature*, No. 1587, vol. lxi., 29 March, 1900, col. 1 of p. 523.

[2] Cobbold, *Veterinarian*, February, 1884.

having worked out the developmental history of the parasite (the so-called *Filaria sanguinis hominis*) and of having also discovered that mosquitoes are the intermediate hosts of the filaria in question," Ross[1] published a statement that the second of these discoveries was made "either on his (Manson's) own initiative or on that of Bancroft." Ross's statement has since been interpreted by Schilling[2] as giving Bancroft the precedence in the significant conception begotten of the discovery.

Before examining this statement it is well to make it plain to the reader that in the history of filariasis the names of two individual Bancrofts occur, namely, Joseph Bancroft, who died in 1894, and Thomas Bancroft, his son; both were contemporary with Manson.

Of these two Thomas was the only one, so far as we can discover, between whom and Manson there was ever any communication. He first wrote to Manson in 1898, for information on the subject of filariasis, and his work—undertaken, as he acknowledges, with "Dr. Manson's encouragement"—was published in 1899,[3] twenty-one years after Manson's discovery had been communicated to the Linnean Society. These observations of Thomas Bancroft, which have been already noticed, cannot therefore be brought into the present argument.

Ross's statement must, of course, refer to Joseph Bancroft, with whom, so far as we can discover, Manson

[1] Ross, "Memoirs," p. 127.

[2] Schilling, "Die Übertragung parasitischer Protisten durch Parasiten höherer Ordnung." (In *Beihefte z. Archiv F. Schiffs-u-Trop. Hyg.*, 1925, vol. xxix., No. 1, p. 312.)

[3] Thomas Bancroft, *Proc. Royal Soc. New South Wales*, vol. xxxiii., 1899, p. 62.

never held any communication, and who, so far as we know, did not himself publish anything on the subject of filariasis but disclosed his observations to Dr. W. Roberts of Manchester and Dr. Spencer Cobbold of London. We must consider that which, but for this statement by Ross, we should have preferred to leave unconsidered, namely, whether the conception of the mosquito as an intermediate host to the filaria may not have occurred to both Manson and Bancroft about the same time and have been expressed by Bancroft before it was discovered by Manson.

The general idea that bloodsucking insects may possess or acquire morbid material and transmit it to man is known to be a very old one. It has been found current among numerous untutored races and even in countries having an indigenous civilization—for instance in India, where the *surra* disease of equines and camels was vaguely supposed to be transmitted by gadflies, as indeed can now be predicated of it in exactly limited scientific terminology.[1] Beauperthuy seems to have been one of the first to try to give some sort of precision to these beliefs about bloodsucking insects, in the case of yellow fever and of malaria. Is there any evidence that Bancroft went beyond this stuff that dreams are made of and rounded off a particular bloodsucking insect as the exclusive agency for the spread of a particular parasitic disease of man? The evidence on this point is decidedly unsatisfactory.

We can find four items of evidence—three by Cobbold, who was in communication with both Manson and the elder Bancroft as a sort of referee or busy publicity agent;

[1] R. Blanchard, Notices Biographiques, xxiv., Louis-Daniel Beauperthuy, 1807–71. (In *Archives de Parasitologie*, vol. xvi., 1919, pp. 503–45.)

and one by Roberts, the specialist on urinary diseases, who writes of Bancroft as his former pupil and to whom Bancroft made his first communication in the matter of filariæ.

"*In April*, 1877," says Cobbold,[1] in a letter of 1878, "Dr. Bancroft had informed me of his expectation of finding that these insects (mosquitoes) suck up filariæ when engaged in their attacks on man. Dr. Bancroft's supposition was a very natural one, but it remained for Manson to make the actual discovery."

"In November, 1877," said Cobbold[2] again, in a Presidential Address of 1880, "Dr. Manson informed me that he had discovered filariæ in the stomach of mosquitoes that had gorged themselves with human blood. Dr. Bancroft, indeed, writing *in the spring of the same year* (1877), had remarked to me that he fully expected the Hæmatozoa would be thus transferred, but his examination of mosquitoes up to the time of Manson's discovery had not been successful."

It would be pedantic to notice the ambiguity and insufficient congruity in the two accounts—one given in 1878, the other in 1880—of the same expression of opinion, were it not that in the interval Cobbold contributed to the Linnean Society,[3] under a very comprehensive title, a garrulous paper purporting to give "the nearest approximation to a correct chronological

[1] *Lancet*, 12 January, 1878.

[2] *Journal of the Quekett Microscopical Club*, vol. vi., 1880, No. 43, p. 58. (The italics in the quotation are ours.)

[3] "The Life-history of Filaria bancrofti, as explained by the Discoveries of Wücherer, Lewis, Bancroft, Manson, Sonsino, myself, and others." By T. Spencer Cobbold, M.D., F.R.S., F.L.S. *Journal of the Linnean Society*, Zoology, vol. xiv., 1879, pp. 356–70.

record that I can offer" of the various links in the discovery of the life-history of *F. bancrofti*. This paper freely awards praise, patronage, and censure among the various contributors to the history, and arbitrates their several claims to distinction and precedence. It gives an inadequate but laudatory account of Manson's work, and a fuller account of Cobbold's communications from Joseph Bancroft up to *20th April*, 1877, in which, however, not a word occurs of any mention of mosquito-agency by Bancroft. This would indeed be a serious omission in a document professing to be both comprehensive and authoritative; and the more so, because on the day when Cobbold presented this document to the Linnean Society he also communicated Manson's original paper—which had been in his hands for at least two months—to the same Society; so that in justice to Bancroft, as well as in corroboration of Manson, an allusion to Bancroft's foresight should have been imperative.

Perplexities in Cobbold's evidence are not dissipated by the evidence of Roberts. It is to be remembered that in 1876, when Bancroft first encountered the filariæ, he referred them to his old teacher, Dr. W. Roberts, F.R.S., the eminent authority on urinary diseases, who in due time sent some of them on to Cobbold. Roberts's account of them is as follows [1]:—"Dr. Bancroft of Brisbane discovered filariæ in the blood of persons suffering from chylous urine in Queensland. He sent me some specimens. I had no difficulty in identifying these with the filariæ described by Lewis. Dr. Bancroft infers from his experience that the disease is not communicated from person to person in the same

[1] Roberts, "Treatise on Urinary and Renal Diseases," pp. 384, 656. Fourth ed., 1885.

family, but thinks that it may be communicated by drinking contaminated water."

We must leave the question to the reader, having justified our action in raising it. The verdict, whatever it be, does not touch the fact that Manson's work was conceived and brought forth in immediate independence of every one but Lewis, whose writings introduced the filaria to his notice, and to whose discovery of the filaria in the blood of man he acknowledged his obligation. The verdict, whatever it be, does not touch the fact that Manson was the first to discover by laborious experiment in a new field that the intermediation of a particular bloodsucking insect as a "nurse" is a *necessity* in the propagation of a specific disease of man—a discovery that threw floods of light into the fields of biology and pathology in a definite direction. The discovery, of course, was based on earlier discoveries: all discoveries must start from the levels of knowledge and with the means of advancement existing at their time. Perhaps the only discovery that might have been made at any time in Man's history, independent of everything in existence at the moment but healthy human social instincts, is the sublime truth that "he that loseth his life shall find it." Manson's discovery started from the established basis that endoparasites like cestode worms and the guinea-worm require, for their persistence as species, an intermediate host. That knowledge provided the base, but it did not point the direction; so little did it suggest a new direction that great authorities like Leuckart stuck to the old base and refused to follow the new direction shown by Manson. There is a mighty difference, both as a biological lesson for future application and as a practical lesson for immediate use, between an intermediary that picks up an infection passively

and by chance in food or water and an intermediary that can acquire the infection only by pursuing and attacking the specific host.

Considering the significance of Manson's work on the *Filaria bancrofti* and the circumstances in which the work was patiently followed to an end so brilliant, it is hard to realize that at the time it did not meet with any signal mark of academic recognition. The highest contemporary appreciation that it seems to have received came from an anonymous reviewer:[1] "Foremost amongst the workers in this field [filariæ and filarial disease] stands the name of Dr. Manson. Dr. Manson's labours have been continuous and incessant, and only those who have watched the development of the whole subject can adequately appreciate its importance in relation to human and veterinary medicine."

[1] *Veterinarian*, March, 1883.

CHAPTER V

At Amoy: Miscellaneous Scientific Work and Retrospect

Manson had worked out his problems, as will have been remarked, by the light of nature. By that light he had seen the value of the comparative method, and was always checking the ways of *Filaria bancrofti* by observations of its congeners in the lower animals. He thus became observant in helminthology, and accumulated some interesting observations not only of other species of *Filaria* but of flat-worms also; and it is with some minor discoveries in this field that we now have to deal, not, however, going too far into details, since they can be found elsewhere,[1] but rather to show his methods and his persistent alertness of mind.

In native dogs he found the embryos of the heart-worm (*Filaria immitis*) and of *F. sanguinolenta* to be extremely common inhabitants of the blood, and although both these species were known to science, he described their parent forms very carefully and followed out the embryonic development of the former and observed the aortic lesions caused by the latter species—small sacculated aneurysms which are capable of obstructing the dog's gullet by pressure. Whether this possibility throws any light on a puzzling stricture of the

[1] Report on Hæmatozoa, in *China Customs Gazette*, No. 13, Pt. 6, xxxiii., Jan.–Mar., 1877, pp. 12–38. Also "The Filaria Sanguinis Hominis." By Patrick Manson. Lond., 1883.

gullet, of which he encountered sixteen cases in a run of 3,055 Chinese patients, in 1876, is a question that might perhaps be mooted but may not be discussed, since a post-mortem examination of a Chinaman was a project in which the investigator risked his own life. As has been mentioned already, he noticed that the embryos of *Filaria immitis* in the peripheral blood had a slacker tide by day—when they congregated in the arterioles of the lungs—than by night. The examination of the ferocious native dogs gave a great deal of trouble.

In the white-necked crow he discovered a new species, *Filaria corvi-torquati*, the parent forms of which live in the pulmonary artery. In this species he watched under the microscope the process of parturition—a peristaltic wave which, starting every few seconds in the uterine horns, and travelling down the vagina, results in the expulsion of twenty to thirty embryos. With a premature contraction, however, a " miscarriage " occurs and eggs are expelled. The significance of this observation was explained in the last chapter.

In the local magpie another new species, *Filaria picæmediæ*, was discovered. In this species the adults were found in couples in a minute tubercle, lying under the endocardium in a pocket of one of the semilunar valves. But even in the field of comparative pathology he was checked by native superstition. " Being denied the opportunity of necropsies in man," he once said to Dr. Sambon, " I had to make use of dogs, cats and birds of various sorts. In the course of these examinations a great variety of hæmatozoal parasites were found. Even in this branch of research native prejudice often proved obstructive. I had found that the blood of the magpie contained at least two

species of filaria, so I procured as many of these birds as possible, but the Chinese told me I must stop my work in this direction, because the magpie is a sacred bird in China, tradition holding that, many centuries ago, the spirit of a defunct Emperor had entered one of these birds. Therefore it was possible that either I or my friends might shoot this particular fowl. I don't know that this absolutely deterred me, but the birds themselves, which, under ordinary conditions, are remarkably tame in China, soon got to know when I was after them."

In the horse he studied the eye-worm, *Filaria papillosa*, and from the fact that a female extracted from a horse's eye was unfertilized, and possessed a remarkable boring apparatus, shaped like an old-fashioned trephine, around the mouth, he inferred that the eye is not the final resting-place of this species.

In domestic fowls he discovered a new species, named by Cobbold *Filaria mansoni* (but now known as *Oxyspiura mansoni*), situated in the nictitating membrane. This parasite he encountered in an unsuccessful investigation of an unknown disease rapidly fatal to fowls and rabbits.

That animal of manifold human interest, the pig, of course came a good deal under the sanitary gaze as a pertinacious scavenger, and in the course of a *Trichina* survey which Manson undertook—although he never met with a case of human trichiniasis—he scrutinized 225 pig carcases. The infection was detected in only two of them, but in one it was so overwhelming as to satisfy him that the danger to man must be rife enough in China, but probably was averted by the customary thoroughness of the native way of cooking pork.

Manson's share in the discovery of the lung-fluke is

a very pretty story of continuance in well-observing. It begins, in 1878, with a patient from Formosa, a Portuguese, who was under his care in Amoy for thoracic aneurysm. Next year the man went home to Formosa, where the aneurysm burst and he died; and as he was not a Chinaman there was no difficulty about a post-mortem examination. The local physician, Dr. Ringer, an observant man, who made the examination, discovered in the lung a small parasite from which a multitude of microscopic "young" escaped, and knowing that Manson was interested in the case he wrote and told him all about it. Thus the first act ended in the discovery by Ringer, in Formosa, of an undetermined parasite whose local habitation appeared to be the human lung.

About nine months afterwards, when Manson was trying to find out where the embryo filariæ got to in the daytime, and in that train of thought was on the watch for cases of hæmoptysis, a petty mandarin who had lived a good deal in Formosa came to him for advice about a certain skin-eruption. The fellow had a harsh voice, and kept clearing his throat, and presently began actually to spit on the floor of the consulting-room. Manson was about to rebuke him for his bad manners when—"My disgust and anger evaporated on seeing that the sputum was tinged with blood. I transferred it to the microscope, and to my astonishment I found, not the expected filaria embryo, but the operculated egg of a different and, to me, a quite new parasite." About the same time Ringer had written about certain minute strange bodies that he had observed in sections of the Portuguese patient's lung. Manson, now suspecting that there might be some connexion between all these facts, wrote to Ringer for further particulars of the para-

site found at the Portuguese post-mortem. So Ringer sent him the parasite, and in the sediment of the spirit in which it was preserved—there were the operculated eggs, like those in the mandarin's sputum. The parasite turned out to be what, since we do not wish to lead any punning reader into temptation, we must call a Trematode. It was subsequently submitted to Cobbold, who pronounced it to be a new species of fluke and named it *Distoma ringeri*.

Unfortunately the mandarin, after once more attending at the consulting-room and providing material for the verification of the operculated eggs, had made an unceremonious exit, possibly being doubtful of the intentions of the foreign devil who had taken such a fancy to his sputa. But before his departure he had suffered himself to be examined and had imparted some particulars of his history. The examination showed that he was thin, but otherwise apparently normal. His story was that thirteen years before, being then resident for some time in Formosa, he had begun to spit blood and had continued to do so at irregular intervals ever since. Thus at the end of the second act, Manson's operculated eggs were definitely traced to Ringer's parasite, and the parasite had acquired not only a local habitation and a name but also a spotted reputation as the cause of an alarming symptom usually significant of grave disease. But the mandarin who could have provided living eggs of the parasite for further investigation had quitted the stage.

(Some months afterwards Manson learned from a private letter and from the *Lancet*[1] that a Dr. Baelz, in Tokio, had described, as being not uncommon in Japan,

[1] *Lancet*, 2 Oct., 1880, and private letter to Manson dated 19 Feb., 1880.

an affection of the lungs signalized by expectoration of sputa containing blood and minute parasites which he regarded as *Gregarinæ*. These parasites proved to be eggs of *Distoma ringeri*.)

E. Baelz to Manson

Tokio, February 19th, 1880.

Dear Dr. Manson,—First of all I have to apologize for not having written to you earlier. I always waited for a good case of the *Hæmoptöe parasitica*, to send you a specimen. At last I have got a prominent one, and trust you will be able to compare the eggs in the sputa with those which you found in the lung of a patient affected with distoma. It is not at all improbable that you will find them identical. I had called them *Gregarinæ*, because they are exactly what is described as such, but when I now send my first notice about them to Europe I saw some cases what seemed to me to prove that the oval bodies must be the eggs of some worm, and if so, there could hardly be question of another than distoma. I wrote this to Leuckart, and he too is of the same opinion. Therefore I am much interested in hearing that the same disease occurs in China.

I have always followed with great interest your publications in the Med. Cust. Reports, especially those about *Filaria*. I have found chyluria in Japanese seven times in five years, which is, as far as I know, not known to foreign physicians. Tokio has about the same average temperature as the middle or the south of France, and so the occurrence of this disease in Japan would show that it is not at all limited to tropical countries. Mosquitoes, of course, we have here too, and so you will not be surprised at hearing that chyluria might be met with in Tokio.—I am, dear Sir, yours sincerely, E. Baelz.

P.S. I ask your pardon for my bad English. I have not much practice in writing the language, and feel only too well that I make always mistakes.

For eighteen months after the mandarin's departure Manson scrutinized sputa, examining about 150 individuals, in a vain search for ova of the lung-fluke,

and at last, concluding that the parasite was not indigenous to Amoy, he had to obtain some infected sputa from Formosa, where this newly discovered endemic hæmoptysis had been found by that time to be common. He was then able to observe the development of the embryo and the conditions under which it hatched from the egg. He found that if the eggs were left in the sputum they decomposed with it: they developed and hatched only if they were carefully washed free of mucus and blood and then kept in fresh water —for a term of six to eight weeks. From this he conjectured that the intermediate host must be an inhabitant of fresh water, and concluded that the rest of the life-history must be unravelled in Formosa or Japan.

(The development of *Distoma ringeri*—now generally known as *Paragonimus westermanii*—has been followed out in recent years in certain fresh-water crabs and crayfish. The adult fluke is now believed to be not exclusively specific to man and known to attack many organs and tissues besides the lung.[1])

Another of Manson's important discoveries—also

[1] "I used to cogitate on how the parasite gained access to the human subject, and thought that by following the destiny of the sputum, the wanderings of the young trematode might be tracked up to a certain point. So I tried the experiment of incubating the eggs of the parasite in water, and for a fortnight or longer made observations on the ova so incubated, but saw no evidence of development. I put the bottles aside and turned my attention to other matters. About six or eight weeks later I found myself with half an hour's leisure, so, before dinner, remembering the bottles with the ova I had placed aside, I took them down and examined their contents. I was interested to find the ova now contained a slowly moving ciliated miracidium."—Manson, quoted by L. W. Sambon, *Journal of Tropical Medicine*, 15 June, 1922.

accidental if it be entirely accidental " to find a sovereign when you are looking for a shilling," as Manson pithily phrased these accidents—resulted in an interesting addition to the list of tapeworms that infest man. In this instance he was in quest of an adult *Filaria bancrofti* which he thought must be in existence in the body of a Chinaman in whom elephantiasis had been concurrent with lymph-scrotum: to find it would be the proof direct to the " retort courteous " of Dr. Tilbury Fox and others. He was determined to get a post-mortem examination, but he had to remember the uproar of the people and the loss of his investment on a former occasion. We are not told how this third attempted and sole completed post-mortem was projected, but much is left to our imagination. The staging must have been gruesome enough for the Resurrectionists; for the work was done in the Chinese cemetery, at dead of night, with only the lantern dimly burning.

The expected *Filaria bancrofti* was not found; but in the kidney-fat Manson came upon a dozen ribbon-worms of a new species, and in a most interesting stage of development. Cobbold described them by the name of *Ligula mansoni*. " Ligula " is now known to be the penultimate (plerocercus) stage in the development of the ribbon-worm, *Bothriocephalus*; and *Bothriocephalus mansoni*, as Manson's spoil of the charnel-house is now called, is now known to be normally a parasite of the dog and cat, and to have its plerocercus or " ligula " stage in frogs and snakes, not in man, although it has now been found in man in sixty instances.

Among many minor matters noticed in Manson's journal is a case of intestinal myiasis in a German sea-

captain, the record being accompanied according to his custom by a good figure.

In the latter half of 1882, after more than six years of engrossing toil since his memorable holiday in 1875, Manson renounced the monastic ties of hospital and laboratory, put off his harness, and bent his course homewards to his native Aberdeenshire. There he settled down for six months on the shooting of Littlewood, by the banks of his own Don, where he gave himself up wholly to shooting and fishing, and became the complete Scottish edition of the British hunting squire to which Guppy had likened him. It is good to picture him, unscathed in body or mind by ardent yet little-appreciated work in a trying climate, forgetting his labour and his toil "among the heathy hills and ragged woods" of his native land; expanding in the pure air and rejoicing in the healthy sports of his boyhood; forgetting Filaria, and liver, and gout, and perchance at times feeling equal even to that supreme test of robust vigour—"the halesome parritch, chief of Scotia's food."

Early in the summer of 1883 he was "owre the sea" again, and back in his Amoy harness; and once more he was at work with the Filaria—upon that confirmation of its metamorphosis within the mosquito which he felt bound to make because of the reluctant reception of his original discovery. It is a pity that he had not seen the suggestion of the able reviewer in the *Veterinarian* for March of the same year—the suggestion that the full-grown larva was set upon its prospective host by the mosquito itself "in the act of biting." He was not a man to sneer at a suggestion. Had he seen it he might have thought again—for he thoroughly appreciated the

boring apparatus of the larva—and might have looked again, and perchance might have given to his discovery the final perfecting touch.

While he was working at Filaria he was also formulating his experience on abscesses of the liver and their surgical treatment, the outcome of which appeared in the *Customs Gazette* for 1883.

But Manson was now nearly 40 years old. The cares of an increasing family were beginning to accumulate. Amoy was undoubtedly a first-rate place for clinical experience and research work, but other responsibilities had to be considered. Large discourse, looking before and after, began to insist very precisely on the mutability of human affairs. So in December, 1883, Manson left Amoy and settled in private practice in Hongkong. Sad to tell, the missionary hospital in Amoy which had been raised to a pinnacle of usefulness and good report by Manson, gradually disappeared, after his departure, into its original obscurity, just as a neglected plantation in the tropics goes back in a few years to the jungle; and then it disappeared altogether, its very site being sold.

(A proposal has been set afoot lately by Dr. Lim Boon Keng, one of Manson's old pupils, to rebuild the hospital as a Manson Memorial, with an Institute for medical research attached.[1])

As we leave Amoy with Manson and look back once more on the scientific work he did there, in isolation, and amid other professional distractions in full measure, we feel inclined to describe it as monumental. Great as is its intrinsic merit, its influence has been something

[1] *Caduceus, Jl. Hongkong University Med. Soc.*, vol. iv., No. 1, 1925, pp. 1–8.

very much greater. For it has been one of the most vigorous factors—though it may not have been the most showy—in reminding the medical profession that medical *science* is rooted and *grows* in biology—a perennial truth which, owing no doubt to a misapprehension of Huxley's views on the place of biology in the medical curriculum, the profession was becoming inclined to ignore. This noble influence of Manson's discovery upon our interesting profession became apparent first —through Schools of Tropical Medicine, of which more will be said hereafter—among practitioners in tropical countries, but since the war it has been spreading generally.

What Manson's posture was when he began his work is modestly described by himself in the following extract from an Address he gave at the inauguration of the Society of Tropical Medicine in June, 1907, that Society being one of the practical outcomings from the experiences of a good many men who, like Manson, had found themselves in positions where great opportunities were obstructed or frustrated by insufficient knowledge of methods of tackling them.

"Thirty odd years ago, and after eight years of experience of medical work in the tropics, during which I succeeded in learning this much, namely, that I knew nothing about tropical disease, I came home on furlough. After a month or two with my people I came to London principally with the view to rub myself up in recent medicine and surgery, more especially in their bearing on tropical subjects. But it was like fishing in a big lake for the two or three fish the big lake might or might not hold. I did not know where the particular fish I wanted lay, and I found no one to tell me where they lay or how to set about hooking them, and I finally

landed at the Reading Room of the British Museum. Dreary enough and profitless enough was the fishing there, as you may imagine."

As we know, Manson did after all hook one good fish there, as he acknowledged, namely, Timothy Lewis's *Filaria sanguinis hominis*.

CHAPTER VI

In Hongkong

MANSON'S motive in leaving Amoy for Hongkong was to better himself: it is, in greater or less degree, the object of all men before, like the Preacher, they have grown old and blasé and have discovered that this too is vanity. Even so, he was not a pipe for Fortune's finger; but he followed his star, and, carrying all his Amoy zeal and experience into the public life of the large European community of Hongkong, he soon gained the general confidence there as a leader of the profession and as a thinker and teacher in larger affairs of public interest.

His good name as a doctor seemed to have gone before him, and he was at once immersed in a private practice that left him little leisure, and that little was soon absorbed in public work. Only occasionally could he find his way to his private work-room, where he again took up the leprosy bacillus, and again came to impotent conclusions with malaria; for he seems to have been unaware of Laveran's discovery of the Plasmodium until after the year 1884, since in that year he was trying—another of what Darwin justified as "fool experiments"—to grow something from malarial blood on sterilized humus, taking his instruction, as usual, chiefly from his own meditation. Later on he heard of and read Laveran's first book and set about looking

for the Plasmodium, but in spite of repeated and prolonged attempts, he failed completely in his search, and at last almost began to have doubts about its existence. Strange as this ignorance and inexperience may appear to-day, it must be remembered that great authorities in England were even then still writing confidently of malaria as an earth-generated poison and speaking with considerable hesitation of Laveran's Plasmodium. Ignorance, however, is less dangerous than error. He still kept an eye on the miracidium of Ringer's Paragonimus ; and as a positive addition to knowledge he discovered at this time a diplococcus in a previously undetermined skin-disease which he then described as contagious pemphigus. He took careful and circumstantial notice of several " new " diseases, and of cases of a disease that had long baffled him before he had recognized it as beriberi, and he was one of the first who now called attention to the cerebral phenomena that sometimes develop with such tragic suddenness and violence in cases of malignant malarial fever. Among minor matters he described a peculiar body, very alarming to inquisitive patients, passed after eating persimmons —a peculiar body which he found could be produced deliberately by the addition of bicarbonate of soda to the meal of persimmons.

It was, however, as a zealous missionary of the medical persuasion that Manson revealed his strength in Hongkong. The profession there being singularly exempt from jealousy, he came to the front, as his first public venture, in the formation of a local Medical Society. His inaugural address,[1] as President, at the Society's first meeting on the 3rd September, 1886, has the true

[1] *Transactions of the Hongkong Medical Society*, vol. i., 1889, pp. 1–25.

missionary fervour. He speaks of the magnificent immensity of the fields of observation in China, and of our imperfect knowledge even of diseases of such common occurrence as fevers. He mentions several diseases of his own daily experience that seemed to have never been described, and in speaking of common parasites he alludes to the fact—true at that time—that the life-history of the common round-worm, *Ascaris lumbricoides*, commoner in Hongkong than anywhere else in the world, was still incompletely known. He boldly states his opinion that for original investigation in pathology and medicine elaborate apparatus is not essential, and backs it up with the historic fact that important discoveries have been made with the simplest means. [This doctrine will, no doubt, be scorned at the present day, when old proverbs are discarded and nothing can be done, from the kindergarten onwards, without special instruments and adroit novel device; though happily there are still some distinguished authorities to remind us of its ancient truth—as, for instance, Mr. Clifford Dobell, who has recently told us how the famous Dutch naturalist, Antony van Leeuwenhoek (1632–1723), with simple lenses of his own making, discovered such minute intestinal Protozoa as *Giardia* (*Lamblia*)—and also how some of that great master's successors, with their much finer apparatus of microscopic investigation, have managed to misunderstand and even misrepresent his discoveries.] He emphasizes the radical weakness of the local profession in the absence of any periodic medical publication in which the experience acquired by a succession of medical men is generally imparted, recorded, and handed down. One very unusual item in the prospectus of a Medical Society was his benevolent suggestion that it should make systematic arrangements for

medical attention to the large class of poor Europeans and Eurasians in Hongkong—a matter that till then had been left to the promiscuous good-nature of the profession. This " country-born " class exists—apart, and in a not always very satisfactory environment, either material or moral—in the background of all big Europeanized cities in the East, unobtrusive and voiceless, but not so entirely listless and incompetent as is too often supposed. It, too, has its own social distinctions, and in the better ranks is many a good man who only needs opportunity—which he so rarely gets —to " break his birth's invidious bar " and hold his own in the world. Manson's solicitude for this unconsidered class does him honour that should not be forgotten.

Another enterprise, of fundamental importance to the European community in which Manson took a leading part, was the establishment of a Dairy Farm for the supply of pure milk, particularly for children and sick people. The necessities of the community in this respect were explained in the following memorandum drawn up by him :

" From a hygienic point of view the milk supply of a community is second in importance only to its water supply. As regards the latter, Hongkong will shortly be in a satisfactory position, but unfortunately, in consequence of the epidemic amongst the cows which the very inadequate, unreliable and exceedingly expensive milk supply has hitherto had to submit to, it seems likely to give out altogether and there is every prospect of a milk famine. This is a serious matter, serious enough for the general public, but especially so for young children and the sick, with whom milk is the principal and often the only food, and is indeed the staff of life. It is felt, therefore, that the present is a good opportunity to establish, by means of a Public Company with an adequate capital, a Dairy Farm, which, while it returns a fair interest on the capital invested, shall supply a thoroughly reliable article and at

such a price that what hitherto has been the luxury of the rich may become, what milk ought to be, one of the principal elements in the food of the poor."

The undertaking was by no means a simple one, since before a herd of European cows could be imported, it was necessary to create a grazing-ground on the steep and bare slopes of the island. When Manson left Hongkong the infant enterprise was fairly started; subsequently it underwent vicissitudes, but it now is a flourishing business concern.

The most eminent of Manson's public services to Hongkong was in the cause of medical education. As at Amoy he had made a sort of vernacular medical school out of the opportunities afforded by the mission hospital, so at Hongkong he turned his position at the new Alice Memorial Hospital to advantage by bringing into existence, in ready concert with his colleagues of the staff and with some additional assistance from the profession outside, a school of medicine within the hospital. The school thus set in motion consisted only of zealous teachers and anxious Chinese disciples, with the hospital as their cosmos, to do what they could in this sort by the working of the Spirit and by word of mouth—trusting to time and the hour for apparatus. The universities of mediæval Europe began in much the same way. Better things have begun in a way no grander. How Manson put his heart into the business; how much prescience and thoughtful contrivance went into it; how he inspired and carried the European community, both official and unofficial, to work with him in it, will be told in the chapter next following. For the present it is enough to say that in these small proportions and short measures, with Manson as sponsor and spokesman, are to be seen the prologue and

precursor of the existing University and Medical School of Hongkong. It may be mentioned also that among the young English-speaking Chinese students attracted to Manson's teaching was Sun-yat-Sun, the future reformer, or revolutionary, who appears to have been particularly successful in his examinations, and of whom more has to be said hereafter. Manson was the main spirit not only in the foundation of the College, but also in the building of the Alice Memorial Hospital in Hongkong (demolished about 1916). Negotiations had been proceeding for years prior to his arrival in Hongkong, but when the financial obstacles seemed hopeless, Dr. Ho Kai (afterwards Sir Ho Kai) provided the necessary funds. Extracts from the minute books (for which we are indebted to Dr. John Anderson) show that Manson had become the Chairman of the Hospital Committee in the autumn of 1886, and on 30 August, 1887, he had already been elected Dean of the proposed College of Medicine for the Chinese. It was through Manson's influence that Li Hung Chang, the great man of China, was approached by the newly formed Senatus to become patron of the College. He replied in the following terms :

To the Authorities of the Hongkong College of Medicine for Chinese

Gentlemen,—I am in receipt of your letter informing me that I have had the honour of being elected Patron of your College. I also thank you for your desire to perpetuate my name on your College walls.

I wish every success to your benevolent design. I learn that there are between 20 and 30 students in the College studying Medicine, and consider it most proper that they should also pay attention to the sister subject of Chemistry and understand how to compound and how to analyse, thus ensuring greater accuracy in the diagnosis of disease and the preparation of remedies.

I remark that your countrymen devote themselves to practical research and base their scientific principles on the results of investigations, thus differing from those who rest content with theories.

The happy results which ever attend the treatment of disease on scientific principles are evidence of the advantage to be derived from the constant study of Anatomy and Chemistry and the consequent illumination of the dark path of knowledge.

There is no doubt that when your admirable project is achieved it will be appreciated and imitated, and that it will, through your students, be a blessing to China.

Trusting that you will prosecute your scheme with unflagging energy, and wishing you my compliments, I subscribe myself on the accompanying card, (Sgd.) Li Hung Chang.

In 1886, the year before the coming into being of the Hongkong College of Medicine, the University of Aberdeen conferred upon Manson the honorary degree of LL.D., being thus the first academic body to show official recognition of his distinguished attainments in science.

In the same year Manson was joined in practice by Dr. (afterwards Sir James) Cantlie. This association relieved him of his surgical work, and particularly of his surgical responsibilities at the two hospitals, where up to that time he had continued to operate and also to work up and publish reports of his surgical experiences and specific methods of operation.

Though never in the best health, and often very seriously troubled by gout, Manson was far too busy to be actually ill in Hongkong. During one of his worst gouty attacks he was suddenly summoned (Nov., 1887) to Tientsin at the other end of China, a sea-voyage of about 1,800 miles, to attend Li Hung Chang, who was reported to be dying of cancer of the tongue. Manson had to be carried on board ship, but on reaching Tientsin

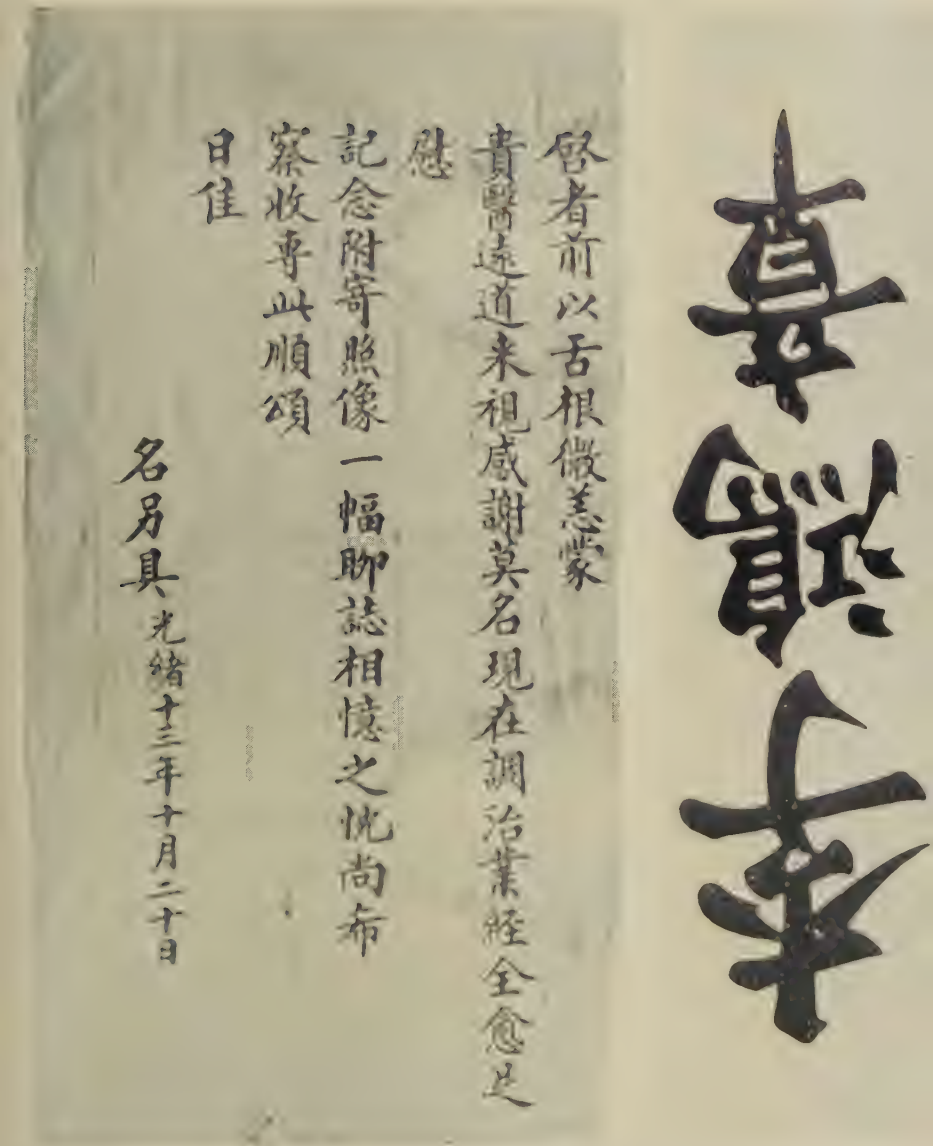

PLATE VIII.—Facsimile of Li Hung Chang's autograph letter of thanks to Manson, with attached visiting card (*p.* 87).

he was able at once to relieve the anxiety of his illustrious patient by recognizing the malady as a sublingual abscess, which was drained and soon cured. On his return to Hongkong he received the autograph letter of thanks from Li Hung Chang here reproduced (Plate VIII), of which the following is a translation :

LI HUNG CHANG TO MANSON

4th December, 1887.

On account of a recent slight malady at the root of my tongue I have had the honour to receive your visit from afar. My thanks I am unable to express, and your treatment has already resulted in a complete cure. Calm, then, your anxiety on my account. I send you enclosed a photograph which may perhaps serve as a reminder of the sincerity of our good feelings toward one another ; and I hope you will accept it. This is the object of my letter and I take this opportunity of wishing you an elegant time. My card is enclosed.

Kuang Hsii, 13th year, 10th moon, and 20th day.

In 1889, after five years of continuous hard work in Hongkong, with hardly any vacant time for his favourite studies, Manson found himself in a position to retire. He had then been in China for twenty-three years : he had not spared himself either at the call of his profession or in his efforts to increase and diffuse professional knowledge ; he was opulent in varied scientific experience, and also had acquired reasonable wealth ; so he now resolved to drop the responsibilities of medical practice and to give his attention wholly to the cognate branches of science. It was natural science that had called him from engineering in his youth, it was the life-history and pathological effects of parasites that had engrossed his thoughts for years at Amoy, and now, in the pardonable weakness of his first rosy moments of freedom, he proposed to make his instinct for natural

science his only taskmaster. Possibly he, like Darwin, might have been able to do it, if he had been actually put to the test.

In March of that year he left Hongkong; but before proceeding to the proposed new pastures of pure science undistracted, he betook himself for some necessary recreation to the bounteous woods and waters of his native Don, where he realized one of the generous ambitions of his youth by renting the fine sporting Lodge of Kildrummy on Donside and came to rest once again amid the beautiful scenes and happy recollections of his early years.

In a retrospect of Manson's overflowing China journals one is struck by the scarcity of reference either to pharmacology or to sanitation. Outside his professional obligations, the things that he thought upon most were the psychology of the patient, parasitology and the causation of disease, and the diffusion of medical education. Here his character is disclosed; for he was one of those humane philosophers whose solicitude is at the sick-bed and who regard disease not as an entity to be overcome by robustious potions, but as a state of deranged nature to be amended by an art that "o'ersteps not the modesty of nature." Such a man will use drugs gently—a specific drug for a specific well-understood purpose—as quinine is used for a malarial fever. Manson himself was probably in advance of his day when in his annual address to the Hongkong Medical Society he spoke in admonition of the amount of quinine annually wasted on continued fevers that are not accurately diagnosed as malarial. He showed the same humane philosophy in his attitude to sanitation. Not —it is superfluous to say—that he had any fundamental want of faith in that important branch of our profession,

or failed to realize that the practical utility of his own greatest discovery lay in its sanitary application, or ever deviated from the time-honoured truth that prevention is better than cure. But he understood in his heart (which is as necessary an instrument as the head in this matter) that the indifference of Oriental and other such peoples to sanitation is not a sort of fault to Heaven, to be suddenly reformed by peremptory official edict, but is an unfortunate state of ignorance quite natural to people whose individual self-interest in the subject of public health has never been awakened. His opinion of forcing all the elaborate apparatus of sanitation upon people who have not been taught and convinced of its significance and value was expressed before the Colonial Institute some years after leaving Hongkong in the following plain terms:

"Unless you get people willing to receive them, there is very little use offering sanitary privileges or trying to carry out sanitary measures. I recollect that an elaborate system of drainage and of model municipal sanitation was supplied to the Chinese. Water-taps, traps, drains, ventilators, and all the rest of it were placed at their disposal. The Chinese turned on the water-taps, but they were too lazy to shut them; and naturally, the supply calculated to last a year was exhausted before the year was half over. The traps had gratings which had been placed over them to prevent them being choked; gratings and traps were ruthlessly removed to facilitate the escape of domestic rubbish. To give these things to Chinamen unappreciative of their purpose and ignorant of their use, was like giving a monkey a fiddle; they did not understand them and they broke them."

CHAPTER VII

In Hongkong: The Hongkong College of Medicine

The first Hongkong Medical College, established on quasi-mediæval lines with Manson as the moving spirit, has already been introduced to the reader. When it had been organized, and classes were assembled, the Senatus invited the attention of the Hongkong community to its existence and to its necessities, since the young academic body, though strong in the spirit and in faith, was destitute of raiment in the form of all the varied apparatus of educational order and precision. A public inauguration ceremony was therefore celebrated in the City Hall (on the 1st October, 1887), the Acting Governor of the Colony presiding, and the hall being filled with the principal residents of the Colony, official and unofficial, European and Asiatic. Encouraging speeches were made, the chief of them being Manson's Inaugural Address as Dean of the new Institution.

We are indebted to Dr. John Anderson, the present Professor of Medicine in Hongkong, for a copy of this Address, which we reproduce in full (docked only of the initial and final complimentary conventions) not for its historic but for its intrinsic interest; because in its vigour, sincerity, originality of thought and expression, and spirit of perfect self-forgetfulness, it is a noble document of the speaker himself; and also because of its wealth of wisdom, and particularly its sensible analysis

of the Oriental mind, it should be rescued from oblivion to be a lay-sermon for young men enlisting for service in our Empire over-the-seas among fellow-subjects of different complexion whom we are too much inclined to regard as mentally "inferior" merely because they are less well-informed than ourselves. For these reasons we give the speech *in extenso*, notwithstanding that its prophetic vision of the awakening of the Chinese giant—made, it must be remembered, at a time when our information about Japan was derived mostly from tuneful mythical sources—appears to have glanced aside in the present perspective.

Moreover, a speech of five thousand words, without a cliché, and with no more than thirty-one appearances of the first personal pronoun—and these mostly in inevitable idiomatic phrase—is something of a curiosity in these days of stereotyped platform jargon and voluble self-advertisement.

MANSON'S INAUGURAL ADDRESS

The Senatus of the Hongkong College of Medicine for the Chinese has asked you to meet us for these reasons: (1) that we may announce the fact that a school of medicine has been established in Hongkong; (2) that we may have an opportunity of explaining to those interested in such matters the object, constitution, and plans of the school; and (3) that we may enlist your sympathy and, at the same time, gain through you publicity for what we believe is an important movement.

In virtue of my office as Dean of the College, I am spokesman for my colleagues.

Although Hongkong has been a Crown Colony since 1831, and its population and prosperity have steadily and rapidly increased; and although hospitals for the treatment of the Chinese have been for years established and are flourishing in nearly all of the Treaty Ports and in many other towns of the Empire, yet, in Hongkong, which ought to be a centre of light and guidance

to China in all matters pertaining to civilization, it was not until this year that a Hospital devoted to the treatment of Chinese on foreign [i.e. European] principles was opened. It is true that before this there were hospitals of a sort. But the Tung Wah Hospital, according to European notions of what a hospital ought to be, is not up to the proper standard, and is, by its constitution and the spirit of many of its directors and supporters, closed to European methods of cure and administration; and the Government Civil Hospital, besides having associations of a kind not pleasing or attractive to the native mind, is too rigidly foreign [i.e. European] in its ways and discipline to suit the great majority of the sick Chinese. Attempts have been made from time to time to supply what was felt to be a public want, but it was not until February this year that they were consummated. As soon as the Alice Memorial Hospital was opened, its erection received its justification; the beds were at once filled, and crowds of out-patients came for treatment. Its success was established within a month of its being opened. In this Hospital the care of the sick devolves on four of the civil practitioners of the town, on a native house-surgeon, and on a staff of dressers or students. To qualify the latter properly to discharge their duties, they require a certain amount of teaching. If we have to teach a few we may as well teach a larger number. The same staff and time will do for sixty as is required for six. Hence has arisen the idea of forming a school of medicine within this Hospital, with these medical men and these students or dressers for a nucleus. The practice of the Hospital is amply sufficient for educational purposes; but as the task of teaching medicine and the associated sciences would be too much for four men, who have other duties to attend to, they have associated with them other teachers, each of whom has a special subject to attend to, one which his previous training and his tastes qualify him to teach.

In order to enable them to receive instruction in English, a preliminary knowledge of that language is demanded of the students. In this respect most of them have qualified in the Government Central School. There appears to be little difficulty in getting students; they are already numerous enough to form a respectable-sized class. After a four or more years' course of study those who come up to a certain standard of proficiency, as tested in written and oral examinations, will receive the licence

or certificate of the College qualifying them to practise in its name. The government of the College will be carried on by the Rector, who hereafter is to be elected annually by the students; a General Council of all teachers and graduates (or as many of them as possible), who will meet once a year to discuss and decide matters of general interest affecting the College; a Senatus composed of the whole of the teaching staff, which will arrange the curriculum and the details of the teaching plans; and a Court, composed of the Rector, a representative of the Senatus, a representative of the Alice Memorial Hospital, the standing legal Counsel of the College, the Dean and a Secretary; these six will form the executive.

Such, then, briefly, is the origin and constitution of the College. The object of it, of course, is the spread of medical science in China, the relief of suffering, the prolongation of life, and, as far as hygiene can effect this, the increase of comfort during life. The desirability of this passes without question; but I can conceive that there may be objectors to Hongkong saddling itself with the task. They may say, why has the attempt not been launched in Chinese territory—in Peking, Tientsin, Shanghai, Canton, or some or all of these or other centres of Chinese life? Well, attempts at medical teaching have been made and are being made at these and other great towns, but hitherto their success has been very partial indeed; and, principally, for the reason that at these places the teachers are too few in number—only one, or two, or at most three—a number quite inadequate for such an undertaking even if backed by enthusiasm and health. That individual teachers have worked very hard and done a very great deal in this sort of work is true. It is only necessary to recall Dr. Hobson's admirable labours; or to point to Kerr in Canton, McKenzie in Tientsin, Myers in Formosa, and many others, less known but none the less noble, to prove that there is no lack of effort. But the effort is individual and not organized. Each little school depends on the health, the energy, the caprice, or the life of one man. Its work may, therefore, be interrupted at any moment and the immature results dissipated. I have known in my time one, or two, or perhaps three men who have thus devoted themselves to medical teaching and medical work among the Chinese, and who to me are the noblest types of humanity. Medical Sir Galahads they are; men who have buried themselves in some

wretched inland town, surrounded by squalor, filth and disease, and there, away from all the comforts they had grown accustomed to, away from companionship, human sympathy and encouragement, have silently, steadily, and more or less successfully pursued their lofty ideal. Such men are careless of praise, they rather shrink from it; they are silent about their deeds, for the most part, or say little or as little as possible in reports. When I have met them and seen or heard of their work, of course I could not help admiring their characters and recognizing the heroic in them, but my thought was "The pity of it!" No good man lives in vain, and no good act is forgotten, but it is a pity to see energy, knowledge, talent, and such power as is diffused by a noble character wasted or at most half utilized, frittered away in individual and unorganized effort. Nowadays, to do effective work even Sir Galahads must fight in disciplined squadrons.

Further, it is necessary not only to have an organized and numerous staff of teachers, but to have also a suitable and extensive recruiting-ground for students. These students must have a preliminary knowledge of some European language. To teach European science, a European language must first be mastered. Translations are not sufficient; they are good in their way and as far as they go, but are miserably insufficient in number and extent. In science, current literature is everything; current literature is not translated, only some of the classics which pass rapidly out of date. In this matter of Medical Education it may also be asked: Why has the Chinese Government not taken it in hand? I do not know. It is sufficient that it has not, and as far as can be seen is not likely to. Now in Hongkong it has been found quite feasible to get a sufficiently large teaching staff together; and, that best institution in the Colony—the Central School—provides us with any number of lads educated in English for students. Here, too, the work of teaching and the fruits of it are away from [native] official caprice and the dangers of persecution, official or popular, that at any time might overthrow a school in China. Here we can receive official protection without interference and, under the ægis of the British flag, science and philanthropy may work and live fearing no man.

We—the Senatus—think, and I trust you and the public generally will agree with us, that the present is the opportunity for Hongkong to take up a manifest and long-neglected duty;

to become a centre and distributor, not for merchandise only, but also for science. I do not doubt our ultimate success, and when we succeed we shall not only confer a boon on China, but at the same time add to the material prosperity of this Colony. For with a large and successful school or college, such as we see in our mind's eye, other things follow. There is many a town in Europe which lives and flourishes entirely in consequence of its being an educational centre, and that, centuries after the commercial or political reasons for its existence have passed away. I do not suppose the sceptre of commerce will ever pass from Hongkong, but her importance and her glory will be greatly enhanced when she becomes a centre for science and letters. He who gives is blessed as well as he who gets. Hongkong may give science and China may get it; but depend upon it the receiver will not fail to recompense the donor in many ways and many fold.

We anticipate that difficulties will attend our steps. . . . We are prepared for troubles, but together we mean to work, and if twelve men with one object steadily in view, and that a good one, cannot give it a lift, we are mistaken. Our principal troubles will not be so much in the teaching of students as in their management and discipline while with us, and in the placing of them afterwards, when their education, as far as we are concerned, is finished. It is hard to say what sort of a being the Chinese Bob Sawyer will prove to be, and we may have trouble in managing and repressing him. But when he has qualified and has left us, then his real troubles will commence, and it is hard to say how he will take them or what will become of him. A few may find employment in Hongkong, some of them perhaps in connexion with the Government—those will have a comfortable time enough; others may find situations in the Chinese public services, but the bulk will have to struggle for themselves into private practice in the towns of the Empire. It is principally on these last that the burden of the fight will fall. For, single-handed, each will have to cope with what we in our conceit call native prejudices. They will have to fight, also, the vested interests of the native profession; and it is quite possible they may be made the butt if at any time official or national feeling should be raised against foreigners. I can see quite well that many will succumb, and give up their profession for something less arduous and less dangerous and perhaps more lucrative. A few will be faithful. These good men and true

will be an ever-increasing army. We will look to them to reform medical practice in China and be the pioneers of science. This is our hope. It is a far-reaching ambition and carries many things with it besides the purely medical.

I can conceive no grander position or opportunity for any man to have than that we offer to each of our students. At his back the whole of European science, before him 300,000,000 to whom to give it. Such a position must fire the ambition of some of them. It is most strange that no great man or great men have arisen for this work. He and they will come. The old Greek cities used to boast of their great men, and claim them with jealous care. Let us hope that in the new and greater China of the future, when the learned dispute of their great men, not a few may be claimed for Hongkong and for the school to-day inaugurated.

No great name is easily made. The task in front of the medical reformer in China is herculean. A successful man in this work will deserve to be called great. There is so much to be cleared away as well as so much to be created. Those who have been even but a short time in the country know what a wretched thing native medical practice is. And no wonder. There is no educational system. It is chosen more as a trade. A man buys two or three books and begins to practise. It is possible that here and there may be a man of great natural talent with special aptitudes who may know something and may at times do good. But the body of the profession, if we may dignify it with such a name, are as ignorant as they are dishonest. The notions on Anatomy and Physiology are absurd; there is no Surgery worthy of the name; medicines they have in superabundance, but there is no knowledge of their action or of the diagnosis or pathology of disease. Hygiene is unknown. The modern medical literature is for the most part but a slavish copy of older works, and the older works but the expression of fanciful theories. We find in them the same spirit of artificial classification, the same love of a "system" that characterized the pre-scientific era in European medicine. For example, as every foreigner soon gathers from his native friends or his servants, every article of food, of drink, and every medicine is classified according as it is considered a heating or cooling thing. In keeping with this, every disease is classified a hot disease or a cold disease. It is evident that a hot disease requires a cooling diet and medicine, and a cold disease a heating diet and medicine.

The delightful simplicity of this has laid a very firm hold on the popular mind. It is a style of logic that catches the uneducated. It has a pseudo-scientific air about it that is taking. It enables the layman, he thinks, to argue with his doctor on an equal footing. It is a sort of medical logic that has often filled the pocket of the European quack, and it is equally productive in China. It is one of those touches of nature we meet everywhere, and proves the kinship of the whole world. By it European medicine is almost invariably judged. To dodge it, so to speak, I have often practised a sort of pious fraud. Medicine in aqueous solution is, like water, considered cooling. Quinine in solution would, therefore, be considered suitable in a malarial fever, but not in a malarial neuralgia. One gets over the difficulty in such a case by giving the drug in pill. I cite this as a fair specimen of the rubbish of which native medical theory and practice is made up.

Whatever may have been the case in the days when China was young, and the national mind vigorous and independent, there is nowadays nothing like original observation and thought, nothing but this " hot and cold " business and such like, a strained reverence for the authority of the ancients, and a persistent effort to make facts tally with fancies. There was a time in Europe corresponding in this respect to the present in China, but it is difficult for us of to-day to understand how the acute and practical Chinese mind clings to and cherishes such a system. That it does, however, is most true. The glamour of tradition, the national reverence for what has been written, the juggling with words which when practised for some time warps the strongest minds, the conservative spirit of the people, the distrust of what is strange—these and other considerations prop up the rotten tree of Chinese medicine, and it will be many a day, I fear, before the younger, more vigorous and European growth will completely take its place. To remove the old and uphold the new is the work our students have before them.

That European science and other portions of our civilization will be adopted by China is certain ; but the rate at which the conversion is to be effected is uncertain. Truth is strong and is sure in the end to prevail ; but the *when* depends on circumstances vastly too numerous and complicated for analysis or description, and most of them beyond the power of any man, or body of men, to control. But there is one thing that has a retarding influence

on the spread of the truth, and not an insignificant one either, and one which is within control. I allude to the *style* in which teachers, preachers, and foreigners generally are in the habit of presenting it. It is too much the habit with us, whether the ordinary lay foreigner or the professional teacher, to carry on our efforts at conviction or conversion in a foolish and insulting manner, anything but calculated to forward the object purposed. We swagger before them and by word and manner say to the Chinese we would have follow us—"You are a pack of fools. All these things you revere are arrant humbug; your ancestral worship, your literature, your talk of filial piety, your paternal Government, your hot and your cold business, all these and many another thing about you are mere foolishness, superstition and vain words. Whereas, look at us—our steamships, our ironclads, our railways, electric telegraphs, industrial machines, parliamentary government, free press and what not. Is not the contrast in our favour? Are we not Gods and you a kind of idiots?" Now, if we think a man is worth converting, it is evident we cannot think him a fool, and when we make the attempt, in the first place it is a great mistake as well as a lie to tell him he is a fool, and in the second place it is a great mistake to show ourselves conceited prigs. We ourselves would give scant hearing to anyone who scoffed at the things we reverence, who told us our heroes were vulgar robbers, our Shakespeare a writer of bathos, Milton a singer of exploded superstitions, parliamentary government a political fallacy, our industrial system a cunning plan to rob the labourer, a free press a form of mob law. Two men, both equally wise, honest, and capable, may have diametrically opposite views on almost any subject not strictly mathematical. Everything depends on the respective standpoints they view it from. As between European and Chinese, the principal difference affecting their respective judgments is antecedent education—using the word in its widest sense. Education in this sense is very much a matter of birth, a thing we cannot control. That we are able to understand and see a virtue in our European civilization is the result entirely of *our* education; that the Chinese do not see with our eyes is the consequence of *their* education. That we see, is no merit in us individually; that they are blind, is no demerit in them. Too often we commit the vulgar error of confounding ignorance with folly, knowledge with wisdom. Ignorance and knowledge are often

accidental, and may call for no blame or praise ; folly and wisdom are personal, and the person is answerable for them. There is many a modern empty head who in the matter of knowledge could give Solomon points. Because he is ignorant, call not thy brother a fool. To my way of thinking, if there is any blame in the matter, it lies at our door. For here we have the highest truths, evident as lighthouses to us, and yet we present them so rudely, so unwisely, and in such a clumsy way to the intelligent Chinese that he will have hardly anything to do with them. I trust our students, when they leave us, will bear these things in mind ; that they will be careful to avoid offending their countrymen by swagger and supercilious contempt ; that they will bear the knowledge they carry with them humbly, and show themselves wise and good as well as learned. In this way they and we will get a hearing. High personal character, a reputation for wisdom, humility, honesty and kindness will often do more to recommend our wares than any quality in the wares themselves.

No matter how we set about it, our task is one of immense difficulty, and unless I had a thorough faith in the science and art we are to teach, and in its ultimate triumph, I, for one, would not be on the staff of this College of Medicine. Other considerations, too, encourage us and beckon us on. Medicine might be called the mother of the sciences ; from her have sprung Anatomy, Physiology, Botany and Biological Science in general ; besides much of Chemistry and a host of subsidiary sciences. As these followed her in Europe, so will they follow her in China. It would be hopeless to introduce them on their own merits, but they can be smuggled in with medicine. Now, Medicine has opportunities to spread and advertise itself denied to those others, and when she enters she will bring them with her. Religion does not despise her aid. The wants of the poor, and sometimes, though rarely, the fears and despair of the rich, give us doctors an opportunity denied to others. The poor are everywhere, they cannot pay for their own doctors, and they come to us, not because they think very well of us ; something, however, they think is better than nothing. Occasionally a rich man, or some of his family, after trying the native faculty in vain, may in their despair come to us. And in China, as in Europe, there is a class of people to whom a novelty has an attraction ; sometimes these come to us, just as if a Chinese doctor began to practise in London he certainly would have a

clientele of eccentrics. The wants of the poor, the despair of the rich, the eccentric in human nature—these give Medicine her opportunity. Gradually we are making good and extending the footing thus afforded to us. Already in surgical matters, that is in disease whose character and method of cure is more or less manifest, we are already acknowledged superior. But in medical disease, that is in disease whose nature is less apparent, the hot and cold system still holds the field. In time this too will be ours, but not till by patient demonstration and multiplied proofs the old fancies are proved to be wrong. Then, too, will the subsidiary sciences be accepted. I suppose, however, this is a question of generations, for, unless in the case of the most powerful minds, rapid conversion to our ways of thinking about and regarding Nature is impossible. It is easier to conquer a kingdom than to convert a parish. The getting rid of inherited ideas is a most difficult thing, and they must be thrown away before the new can enter. For the last hundred years in Europe the struggle to cast off old fancies has been as vigorous as that to acquire new knowledge. Perhaps it is the most important part of the wonderful activities of recent years. And it is still proceeding. These old clothes are the greatest hindrance to the advance of science. There is nothing science desires so much in her votaries as an empty mind; a *tabula rasa* on which to marshal facts. The mind should be like the blackboard of the schoolroom before the lesson begins. In Europe, from witchcraft upwards and downwards, there was a high accumulation of the rubbish of the ages, and there are still many daubs and splotches of tradition and superstition to be wiped out. In China there is not an inch of the board unoccupied by vain tradition, silly theory, or superstition. These have to be one by one expunged. Surely this is a work of generations. Our little contribution looks small in contrast to what has to be done. We make it humbly but hopefully.

Twenty odd years ago the Taiping rebellion was suppressed. Those of us who were in China then thought that surely now a new era is beginning; and we waited from day to day and year to year for what is called the opening of the country. In imagination we saw a network of railways and telegraphs rapidly spreading itself over the land; hospitals and schools of science rising in the towns; public works going on everywhere; there was to be better government, better food, better clothing, better houses,

more security and more comfort for every one. Everything was to be squared to the foreign model, and we foreigners were to "boss" things generally, making our fortunes the while. The desirability of all this was very manifest to us, and we could not understand why it was not set about immediately. In our disappointment we took to calling the natives names; we called them fools, and we had worse names for their rulers, for we thought them not only ignorant, and unwise, but, from highest to lowest, traitors and dishonest. By degrees opinion on these points is veering round; mine at all events has veered completely, and I no longer regard people and mandarins as I did when I was half my present age. The fact is, though we thought reform was desirable, the people, those who had to pay for it and whom it most concerned, did not see with our eyes, and their mandarins, whether from policy, ignorance, venality, want of patriotism, or a fear for their own order, would not compel reform. In my opinion the judgment of the people and the action of the governing classes was correct. I do not suppose that at first this judgment and this action, or rather inaction, was the outcome of any intellectual operation of the nature of statesmanship, but we may be pretty sure that it has been the means of consolidating and increasing the strength of the Empire and enabling it gradually to accommodate its huge bulk to the altered circumstances of to-day.

The outcome of it is that China is changing front gradually and all along the line, and while doing so has avoided disintegration and revolution, has compelled respect, and has kept foreign fingers out of the pie, until now, when no foreigner dare interfere with her unless prepared to get his fingers well burnt. Whether the wonderful feat has been effected by the skill of the Peking Government, by the sagacity and influence of their foreign advisers, by the elasticity and adaptiveness of the radical principles of the Government, or by that Providence that takes care of the crustacean when it casts its shell, I cannot say. Sure enough, the despised Celestial is effecting an unprecedented feat and is passing from ancient to modern, and not a drop of blood has been spilt in the process. There has not been, nor apparently will there be, any sudden reform, and therefore no sudden revolution or other cataclysm. The old lines of government that have stood the test of thousands of years are broad enough to take in even railways

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and telegraphs, ironclads and machine-guns. By degrees what of good we foreigners have to offer will be introduced from above downwards and below upwards. The Government and governing classes, in order to strengthen their own power over the people and protect them against foreign enemies, are gradually reforming their military system, which includes the introduction of the means of rapid communication and careful management of their revenues. This means the introduction of much more than appears on the face of it. From below upwards, however, will proceed the greatest reforms. From below upwards will spread revolution in thought, in the way men regard religious, social, scientific and political matters. Slow at first will be this progress. These cautious, conservative, clever people have to be convinced before they are converted. Time is necessary for this. Until they understand, and understand it in all its bearings, the Chinese will not adopt a new thing. They are in point of sagacity, as well as size, the elephant among nations. They will not make a new step until they are sure the ground they would tread on is firm and reliable. In my opinion they are wise in this. Changes ought to be slowly made, thoroughly digested, and thoroughly assimilated before others are undertaken. This the Chinese do. They do not object to change, but they must know all about it first. Slow reform, the outcome of education and conviction, spreading principally from below upwards, is what is going to change the face of this land.

And when these reforms are effected and changes made, what to us Europeans will be the consequences? Politically, over three hundred millions of the most industrious, thrifty, persevering, clever people, at present a cypher in political calculations, will be no insignificant factor in the combinations of the future. Their numbers—no longer kept under by preventable disease, civil wars, infanticide, or polygamy—will rapidly increase, and they will expand in all directions. Perhaps the deadly upas tree of opium will by that time have been uprooted. Minor peoples, Annamites, Siamese, Malays, will go down before them or be absorbed by them; a great homogeneous Chinese-speaking nation will stretch from Siberia to Australia. It requires little of the prophetic to foresee this; the process has already commenced. In these days wise men will again come from the East. The people who gave us the invention of printing will give yet other

useful and peaceful arts; the first to use gunpowder will not be backward in the art of war; the discoverers of inoculation will add again to the prevention and cure of disease. Those hundreds of millions will double the recruiting-ground of science and may yet give back to Europe more than they got. It seems to me sometimes that we are teaching the Chinese to beat ourselves.

However this may be, it appears to us that the duty that at present lies next our hands is this matter of the spread of European Medical Science. I have described, very lamely I fear, the origin of the movement, our plans, our objects, and our hopes. There is yet one other thing I would allude to before I sit down. We have students, we have teachers, we have an organization, and we have a name, but as yet we are, I am sorry to say, without a local habitation. Very soon class-rooms, laboratories, museums and libraries will be required; this means a large building and endowment. In olden times the housing and endowment of letters and science was considered by princes their peculiar care and privilege, as the names of many a King's College or Queen's College and other honourable titles testify to this day. In these modern days the princes of the land seem to have other uses for their money—at all events they have, for the most part, abrogated this one of their princely privileges. Nevertheless, other princes have taken up what those of the land have laid down, and the princes of commerce and finance have nobly stepped into the gap. In America, in England, in our Colonies and in India, these generous, far-seeing men are doing kings' work, and every year some great lighthouse of learning rises at their bidding to help to illuminate the world. Nor are we without indications that such a man or men are or will yet be found in Hongkong. When he builds and endows us, he will raise a monument to himself more enduring than brass; and, better far than this, he will have done *more* than his duty; and men will say of him: "He has deserved well of his country."

CHAPTER VIII

In London

THE respite from labour to which Manson had looked forward was destined to be a short one. He rented for a year the estate of Kildrummy, on Donside, noted for its sporting facilities, and deriving its name and fame from an ancient castle associated with Margaret, sister of Robert the Bruce, and here during the winter season of 1889–90 he indulged in the pastimes of a country gentleman, and well maintained the reputation he had earned as a skilled game shot. He had returned from China a comparatively wealthy man, the result of successful practice and judicious investment, but in the summer of 1890 he found himself compelled, owing to depreciation of the Chinese dollar, to embark on practice in London and settled down at 21 Queen Anne Street, which became his home for the next twenty years. To re-commence his career at the age of 46 as a consultant in London, whither he came unheralded and practically unknown, was tempting Fortune. It was not surprising that practice did not come very readily. There were some lean years to be lived through, but in time the fame of his former achievements, enhanced by many further important discoveries, bore fruit and brought him well-deserved rewards.

There was one feature of 21 Queen Anne Street

(which has been demolished some fourteen years) which merits a brief description. This was a sanctum, familiarly known as the "muck room," which was destined to become the nucleus of the future London School of Tropical Medicine. On a rude bench by the window the visitor might observe a few bottles of stain, a watch-glass or two, some dishes, microscope slides and cover slips, while near by stood an unpretentious-looking microscope, whose somewhat unkempt external appearance was hardly an index of its efficiency. He believed, and frequently accentuated his belief, that elaborate apparatus may be rather a hindrance than a means to good work, but he took every care that the lenses of his microscope should be the best procurable. On the shelves the visitor would observe glass jars containing mosquitoes, and sometimes there were birds in cages whose blood might contain interesting parasites. Then, from time to time, he kept tame rats and, on occasion, one of these would escape and cause consternation in the household. Along the walls was ranged a comprehensive medical library, while a roomy cupboard held a heap of miscellaneous and dust-covered literature. The room was in a state of perpetual disorder, yet, however primitive the conditions appeared to be, he managed to obtain beautifully prepared specimens. In this room he would work far into the night. Often, it is recorded, he hardly went to bed at all. In spite of the absence of those bountiful opportunities for research which he had formerly enjoyed in China, he still succeeded in finding in London material for the study of blood parasites. In order to do so he entered into arrangements with various Missionary Societies so that he might obtain blood films from natives of Africa and elsewhere. Convinced that in that continent there

were other blood parasites of importance to be discovered, he engaged in correspondence with Dr. Grattan Guinness, who was at that time working on the Congo, and through the agency of that gentleman some natives were sent to England for investigation. In November, 1890, whilst examining the blood of a native who was suffering from sleeping sickness, or "negro lethargy," and was under the care of Stephen Mackenzie of the London Hospital, he discovered some filariæ which interested him. Shortly afterwards another patient whose blood contained filariæ became insane and was consigned to Hanwell Asylum. This patient had an interesting history. Sent over, at the age of 14, from Africa to England and America to be educated as a missionary, he developed delusions, believing that human flesh was being given him as food. The periods of somnolence between the paroxysms of excitement led Manson to think that the patient might be suffering from sleeping sickness, so, ever eager to find the cause of disease, he paid many visits to the asylum to examine his blood, and on one occasion he came within an ace of losing his life. "I had just placed a blood film under the microscope," said Manson, "and was adjusting the focus, when the lunatic, shouting out, 'Have you not had enough yet?' snatched up the microscope and, raising it above his head, was about to brain me." Manson was saved by the Superintendent of the Asylum, who promptly grappled with the maniac until attendants, hearing the scuffle, ran in and removed him violently shouting and struggling and smashing everything within reach.

At the end of his first year in practice in London, after applying himself for a few brief weeks to restudying anatomy and physiology, Manson was admitted to the

membership of the Royal College of Physicians after examination.

In May, 1892, came his chance in London. There occurred a vacancy in the office of Physician to the Seamen's Hospital Society. Manson and two other candidates for the post were considered to have such high qualifications and recommendations that they were all invited to appear before the general committee of the society. In the final ballot Manson received the whole of the votes, and so was unanimously elected.

Thus began a long and intimate association with the Seamen's Hospital Society. Manson had in his charge a ward of some fifteen beds at the Albert Dock Hospital, situated quite close to the docks where the shipping from the tropics was berthed, and in his beds were to be found lascars, sidi-boys, Chinese, negroes and representatives of other races. It was also part of his duty to visit, at frequent intervals, the parent establishment, the Dreadnought Hospital at Greenwich. Manson realized, as none of his predecessors had done, that here lay an opportunity for the investigation of tropical diseases hailing from all quarters of the globe, and he was not slow to avail himself of it. He was now to be observed in all weathers travelling three or four days a week down the weary maze of streets, an hour's journey or more, which lead from the West End to the Royal Albert Dock, and, having established a small laboratory in the vicinity of the hospital, he would linger there till late at night, studying the specimens obtained from patients in his wards.

His reputation as a teacher of clinical medicine in London was advanced by his demonstrations of the life-history of the guinea-worm, examples of which were often encountered in his wards (*see* p. 193). The

ingenious experiments he had devised to show the adaptation of the parasite to its surroundings began to attract medical graduates to this out-of-the-way hospital on the very outskirts of the metropolis. The peculiar habits of this curious and somewhat formidable parasite are best described in his own words:

"The female guinea-worm has a good knowledge of anatomy, for when at maturity she moves down to the leg, without wounding the blood-vessels or nerves, and rarely strays into the joints. In due course, when ovulation is completed and her embryos are ready for temporary independent life, she drills a little hole in the derma, but does not penetrate the skin. Over this hole a small blister or bulla forms, perhaps half an inch in diameter. By and by the bulla bursts, or is ruptured, disclosing a superficial circular erosion in the centre of which may be seen the minute hole leading to the worm, or from which half an inch or more of the head of the worm may be seen protruding. . . . If, at the time of her appearance at the surface of the body, you manage to procure an uninjured guinea-worm and dissect her, you will find that from head to tail she is little more than one long tube packed with young. To accommodate the millions of long-tailed embryos nearly every organ of her body has been more or less sacrificed. Although thus devoting herself practically entirely to reproduction, the guinea-worm is nevertheless at this stage in a grave obstetrical dilemma. In the process of uterine expansion her vagina has become obliterated. Moreover, owing to her position at the surface of the body, if by any means she should not succeed in expelling her young, most probably they would perish from want of their proper element, water. Here, then, is an organism packed with an enormous brood of water-demanding embryos without a contractile uterus and far away from the element her young require. How does she manage to launch her brood so that they may obtain a chance in life and so continue their species? Some people may think that it is wasting time to describe trivial helminthological details, but this is not so. The same laws, the same logic, apply to big and little parasites alike; but they are more easily perceived, more readily grasped and studied in the case of the larger organisms. Let us suppose a guinea-worm has broken cover about the dorsum

of a patient's foot. As a rule, all we see is a small circular erosion with a minute hole at its centre. Now place the patient's foot in a good light, and in a position favourable to close observation. Fill a sponge with cold water and, squeezing the sponge gently, get a trickle of water to fall on the foot some distance, say a couple of inches, from the little sore. The water need not, and for our purpose should not, touch the sore. Meanwhile watch carefully the little hole. In less than a minute, the cold water trickling on to the foot the while, there will suddenly well up from the little hole and overflow the sore a few drops of milky-looking fluid. In ten or twelve seconds this flow will cease, and it cannot be reinduced until after some hours, when a fresh application of cold water will again provoke it. Take up in a watch-glass some of the milky fluid and place it under a low power—an inch objective —of the microscope. It is a mass of embryo guinea-worms, each worm bent on itself with its long slender tail sticking out at a tangent from the ring in which the body is disposed. There is very little movement beyond now and again a wag of the tail. Now add some water to the watch-glass. In a few seconds the little worms extend themselves and begin to swim about, at first slowly, then with increasing vigour until, finally, the preparation is a seething mass of wriggling worms—a most remarkable sight. Manifestly the young guinea-worms are in their proper element. They will live thus for eight or ten days, some of them, especially in muddy water, even longer. Evidently we have assisted at the parturition of a guinea-worm and at the introduction of her young to their future career.

"But, you may ask, how has the worm managed to get over the difficulty of the absence of a vagina which had become obliterated, and the absence of contractile elements in her uterus, which was necessitated in order to accommodate the prodigious swarm of embryos with which she is packed from head to tail? If you have patience and visit your guinea-worm daily, and douche the patient's foot daily, keeping the worm from injury in the intervals by covering the sore with some simple non-poisonous dressing, very likely you will get your curiosity gratified, and will witness a display of obstetric surgery not to be beaten even in the most advanced clinics. Sooner or later, at one of your daily visits, on removing the dressing you will find that the worm herself has partly emerged from her hole, half an inch or even more of her head protruding.

Now repeat the douching and watch the head carefully. Presently a beautiful delicate and pellucid tube is slowly projected from her mouth to the extent of three-quarters of an inch or thereabouts. At first, the contents of the tube rapidly become milky, evidently from milk material being forced into it from behind. The little tube now becomes very tense. Suddenly it ruptures, collapses, and the milky fluid containing the embryos is spilt over the sore and surrounding skin. A wonderful thing this. Parturition has been effected in response, be it observed, not to a stimulus applied directly to the worm herself, but to a stimulus—the cold water—applied to the skin of her host, with whom she has no organized connexion. Somehow this stimulus has induced contraction of the musculo-cutaneous coat of the worm, which thus comes to function as a uterus by forcing the uterine tube with its contents through the mouth of the worm. When the prolapsed uterus ruptures, the protruded and now collapsed portion shrivels up into a mere thread, the rapid drying of which effectually and firmly closes the wound for the time being. But on reapplication of the stimulus of the cold douche some hours later, a fresh portion of the uterine tube is protruded, ruptures and collapses, once more securing another batch of embryos. And so the process goes on, until inch by inch the entire uterus is expelled and parturition is concluded."

This description may be taken as an example of Manson's delight in tracing out the mechanism of natural processes in the lowly forms of life, and of his precise and minute observation, as well as of his felicitous style of writing.

In 1892, while still engaged in his study of blood-worms, we find Manson in correspondence with Roger Casement,[1] who was then stationed in Sierra Leone,

[1] Casement was at this time in the Consular Service, and afterwards was H.M. Consul-General in Rio de Janeiro. He was first brought into prominence by his investigations of the Congo and Putumayo rubber atrocities, which had greatly troubled the public conscience. He retired from the Consular Service in August, 1913, and became involved in Irish politics, to his own eventual undoing.

where he was taking a close interest in ethnology and native habits. The correspondence appears to have been concerned mainly with the subject of enlarged lymphatic glands, so often found in association with sleeping-sickness. To remove the cause of the disease, which was believed to reside in these structures, it was customary to excise them in a very primitive fashion. "The medicine man," wrote Casement, "after feeling the hard lumps embedded in the muscle round the back of the neck and in the glands of the throat, worked them up clear of the veins and then, with a rough country-made lancet, laid them open to view." A needle was passed through each lump and, the thread being drawn after it, a loop was formed and the hard white masses drawn out of the flesh and muscles. Five or six such operations were performed and the wounded parts wrapped up with a covering of leaves and palm-oil and bandaged to keep out the cold. Strong native purges were then administered during one or two days, to clear thoroughly all the impure matter out of the blood.

Thus we find Manson taking an active interest in sleeping-sickness—and, though up to that time he had never visited the Dark Continent, he was destined to play a part in its final elucidation, as we shall see in Chapter XI (p. 198).

The story of Manson's mosquito-malaria theory is told in the next two chapters, but it may here be recorded that by 1893, before formulating that theory, he had become familiar with the malaria parasite in all its stages within the human body. Though discovered some twelve years previously, the appearance of the plasmodium under the microscope was still sufficiently novel to arouse much interest; and there were not

wanting those who still doubted its very existence. In December of that year he gave a demonstration in the wards of University College Hospital on the parasite. The patient's blood contained the remarkable boomerang-shaped "crescent" form of the parasite. Considerable clinical interest centred in the case from the fact that he was thought to be suffering from a bad attack of "ordinary dysentery." Being puzzled by the high temperature which accompanied the dysenteric symptoms, the house physician had placed some of the patient's blood under the microscope and was soon able to demonstrate crescents.

By this time the subject of malaria had attracted general attention, and, as a guide to the more accurate study of the disease, Manson had drawn up a malaria chart, in which he recorded the types and varying phases of the malaria organism parallel with fluctuations of the body temperature.

In 1894 Manson began to give public lectures on tropical diseases in London, first at the newly founded Livingstone College in Leytonstone to missionaries proceeding on tropical service. Later in the same year he was gratified at being appointed to give lectures on the same subject at Charing Cross and St. George's Hospitals. "I am full up with preparing lectures for my course on tropical diseases at St. George's," he wrote to Ross. "I hate the labour of writing, but I am in for it and have got to get it done somehow."

It was always a matter of great regret to him that he was unable at this time to leave London for the tropics in order to put to the test in some suitable region his theory of the life-history of the malaria parasite in its cycle outside the human host. With this purpose in view he applied in 1895 for what now seems the modest

grant of £300 to enable him to proceed to British Guiana to work out the hypothesis which he had formulated.[1] The application was only partially successful, the recommendation in the Minutes of the Finance Committee of the British Medical Association for this year being that "Dr. Manson be granted £150 to assist him to proceed to the West Indies to study the malaria parasite." Why the commission was not accepted is explained in the next chapter (p. 129).

Whilst preparing the Goulstonian lectures for the Royal College of Physicians on the "Life History of the Malaria Germ outside the Human Body," he was anxious to obtain a filaria-infected mosquito for microscopic section and had written to Ross in India and to friends in British Guiana to obtain such a specimen, but without success. In a letter to Ross dated 23 December, 1895, he related the following remarkable story:

"I have at last succeeded in getting a filariated mosquito. Many years ago—seventeen or eighteen—I sent to Cobbold a lot of filariated mosquitoes. I knew that Cobbold's collection had gone to the College of Surgeons, and so got permission to look over them for my mosquitoes, but failing, I went to Stephen Mackenzie, and there found a small bottle with a solitary mosquito floating in glycerin. In a section of the blood in its abdomen there were my Amoy filariæ, and in its thoracic muscles they were too, most beautiful to behold."

Although Manson frequently protested his inability to draw, he had acquired a considerable degree of draughtsmanship. In China he was wont to illustrate

[1] The concluding paragraph of his famous hypothesis read as follows:—"The hypothesis I have ventured to formulate seems so well grounded that I for one, did circumstances permit, would approach the experimental demonstration with confidence."

with his own pencil his earlier papers on filariasis, and some of his drawings of filarial development in the mosquito are reproduced in Plate VII (facing p. 60). The illustration of the terminal-spined egg of the Bilharzia parasite (Plate IX) shows that the low estimate he had formed of his capacity for drawing was by no means correct. The delineation is good, and the detail is remarkable for its accuracy.

In October, 1896, the capture and detention of Sun-yat-Sun (or Sen) in London created a political sensation. It will be remembered that Sun-yat-Sun was one of Manson's earliest students at the Medical School in Hongkong (1887–92). Attempting to practise his profession in Macao, a Portuguese settlement near the mouth of the Canton river, he soon found himself in difficulties with the Portuguese authorities, and had to leave. He had already formed an association with the leading spirits of the young China party, and, migrating to Canton, he soon became involved in revolutionary activities against the Central Government, in which he was aided by a number of discontented soldiers, but a military movement which aimed at destroying the Canton Government by force failed, and Sun had to flee the country. Travelling viâ America, he eventually reached London on 1 October, 1896. One evening shortly after his arrival he was dining at Manson's house, and the latter jestingly warned him to keep away from the Chinese Legation, little recking how pertinent this warning might prove to be. On 11 October, 1896, while walking down Devonshire Street, Sun was approached by two Chinamen, who enticed him into the Chinese Legation in Portland Place, and there he was detained for several days, apparently in fulfilment of orders received from Peking commanding the Legation

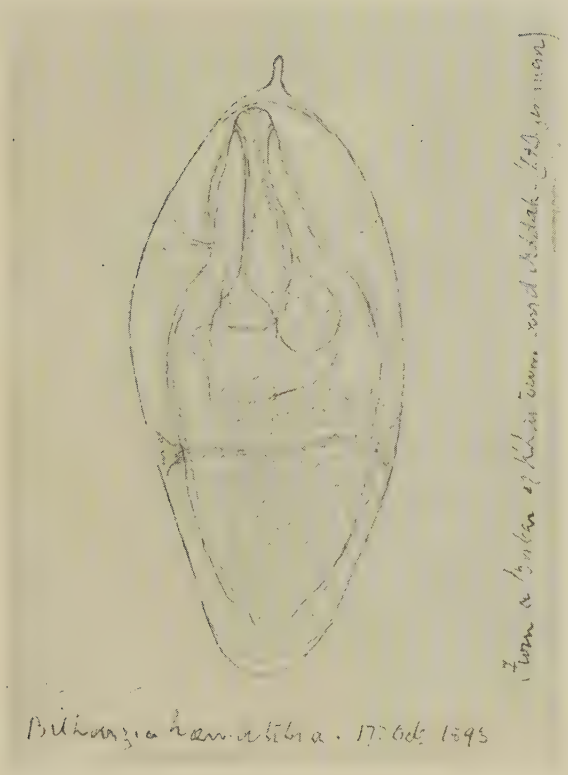

FIG. 1.—Drawing by Manson (1893) of the terminal-spined egg of the Bilharzia parasite.

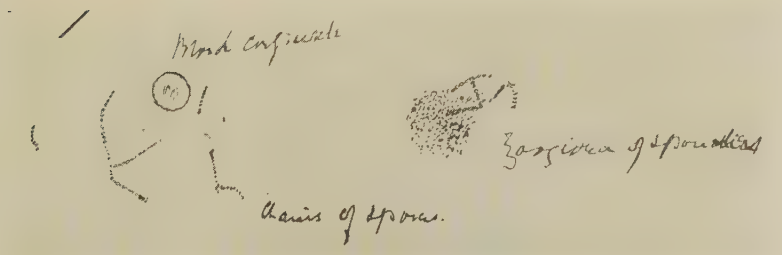

Fig. 2.—Manson's drawings of a bacillus discovered by him in cases of leprosy in 1879, about the same time as the demonstration of the leprosy bacillus by Hansen (p. 29). (*From his Diary.*)

PLATE IX.

authorities to hold him and ship him back to China as quickly as possible. Fortunately he was able to enlist the sympathy of one of the English servants at the Legation and slipped into the coal-scuttle a note addressed to Manson, who received it on 17 October. With Sir James Cantlie, Manson at once approached the police authorities, but without effect. On the following day he visited the Foreign Office and interviewed the Resident Clerk, but being unable to obtain satisfaction, he boldly presented himself at the Chinese Embassy and demanded to see Sun, but was told that he was not in that building. Manson told the door-keeper that he was well aware that Sun was being detained and that he wished to inform the Legation that the British Foreign Office had been made cognizant of the fact. Hearing that Sun was to be smuggled out of the Legation during the night and hurried away to a ship waiting in the London Docks, Manson and Dr. Cantlie hired a cab and posted themselves outside the Legation, waiting there all through the night. The next day Manson once more visited the Foreign Office, this time to interview Lord Salisbury, who at once insisted on the captive's immediate release. The eventual return of Sun to China and his activities as a revolutionary in that unhappy country until his death in 1925 are now matters of history.

By Manson's appointment as Medical Adviser to the Colonial Office in July, 1897, his status was sensibly enhanced. No other position to which he might have aspired could have given him such an opportunity of promoting his scheme for special education in tropical medicine. The appointment, confirmed by Mr. Chamberlain, the Colonial Secretary, on 14 July, was as welcome as it was unexpected and fortuitous. Happening to meet a friend, he was informed that applications

for the post were being invited. At Whitehall it was stated that the list had been already closed, but that an extension of time might be made in his favour in order that he might collect credentials. This exceptional opportunity was due, it seems, to the personal intervention of Mr. Chamberlain, who had already heard of Manson and his work. The close association between the two men which was thus initiated was destined to be fruitful of great results. It led to a recognition of the fact that successful colonization in the tropics, more especially in Africa, depended upon the preservation of the health of the officials and of the agents of commerce. One of the firstfruits of the new policy was the reform of the medical reports from all those Colonies not having responsible government. It was suggested that all reports should be sent in on a definite date and be published annually as one volume, which would be circulated in the Government service, presented to the more important medical libraries and placed on sale. Further, it was thought advisable that Colonial medical officers should be encouraged to contribute special papers on the diseases prevalent in their respective Colonies, so that, after due editing, they should appear as appendices to the annual reports. It appeared to Mr. Chamberlain that by working on these lines the value of the medical information received from the Colonies would be much increased. The members of the Colonial Medical Service would thus be brought into correspondence with one another, while useful knowledge acquired in one Colony would be quickly disseminated, and encouragement given to work and to friendly competition, resulting in time in important additions to medical science.

The organization of the Medical Service in West

Africa with a properly constituted West African Medical Service recruited from specially trained medical officers, drawing a higher rate of pay and emoluments than they had hitherto enjoyed, was brought into effect at this time. The outcome of this reorganization was not evident for many years, but it is now acknowledged that to it is due the greatly improved health conditions which now obtain on the West Coast of Africa.

In 1897 Manson bought a cottage at Chalfont St. Giles, in Buckinghamshire. There he spent his week-ends and was able to indulge in his two hobbies—gardening and carpentry. In this retreat he was able to banish from his mind professional worries, and would refuse to discuss any medical topic whatever. It was this power of entirely freeing his mind from oppressive thoughts that probably maintained his mental alertness to an advanced age and kept him capable of so much sustained work.

The year 1898 appears to have been an especially busy one, for in addition to his other activities he was now appointed Lecturer on Tropical Diseases to the Royal Free Hospital for Women, a post he retained till his retirement in 1912. The appearance, in 1898, of his Manual of Tropical Diseases, on which he had been engaged for two years, was a landmark in his career. Its publication did much to advance his reputation, and it soon took its place as a standard work on the subject. During the next twenty years it went through six editions, and was reprinted many times, besides being translated into the French and Spanish languages. In addition to his official duties at the Colonial Office, which were at this time particularly heavy, he was engaged in the prosecution of his malaria studies in conjunction with Ross, and published many papers on

this subject, culminating in his announcement at the British Medical Association meeting at Edinburgh, in July, 1898, that the problem had been partially solved. For this occasion he had organized a Tropical Section of the Association, at which he was able to demonstrate some of the intermediary stages of the malaria parasite within a mosquito from specimens specially dispatched for the occasion by Ross. This exhibition in itself was a sufficient justification for instituting a special Section for Tropical Diseases.

In 1900 Manson was elected a Fellow of the Royal Society, and was made President of the Epidemiological Society. He had the satisfaction of witnessing the complete vindication of the truth of his malaria theory in the success of the Campagna experiment and in the experimental reproduction of malaria in his eldest son,[1] and in June of that year there appeared in the *British Medical Journal* the paper by Low in which the exact manner of transmission of *Filaria bancrofti* by the mosquito was placed beyond doubt. So the seal was set upon his early Amoy work on filariasis.

In 1902 Manson received a shock from which it took him a long time to recover. His interest in beriberi had led him to organize an expedition to Christmas Island in the Indian Ocean, where an epidemic amongst Japanese labourers had recently broken out. The expedition left in November, 1901, and after a period of great activity its investigations were brought to a close by the sudden death of his eldest son.[2]

[1] *See* post, p. 180.

[2] Patrick Thurburn Manson, born in August, 1877, at Amoy, was educated at Harrow and Guy's Hospital, where he was noted for his athletic prowess. After studying pathology at Aberdeen, he returned to work in the London School of Tropical

In the following year (1903) Manson was knighted, and in 1904 the University of Oxford honoured him with its D.Sc. degree. In 1905 he received an invitation to lecture in the United States; this engagement came as a welcome interlude to a busy professional life. Accordingly he travelled to San Francisco to deliver in August and September of that year the Lane lectures at the Cooper Medical College. These took the form of a series of addresses on the main types of tropical disease, and were subsequently published in book form. Felicitous and somewhat racy in style, they were written during the few days occupied in crossing the Atlantic—an example of the rapidity with which Manson could write when occasion arose. The lectures were greatly appreciated in California and undoubtedly gave a stimulus to the study of tropical medicine in the United States.

Of his experiences in America he regarded his encounters with enterprising newspaper men as the most entertaining. At Chicago, for example, he fled to his apartments followed by a horde of reporters, locked himself in and retired to bed. Next morning he was astonished to read in the *Tribune* an article under his name describing Chicago as the most unhealthy city in the universe!

At a meeting at the Colonial Office in May, 1907, to arrange for the formation of a Society of Tropical Medicine, Manson was voted to the chair and became President of the Society, which flourished greatly under his influence during the next two years. As President,

Medicine and was house physician to his father at the Albert Dock Hospital. By his education, training and keen scientific spirit, he was well qualified for research work such as that which he was prosecuting at the time of his death.

he set up for his successors a high standard. He rarely missed a meeting, and by universal consent he filled the position with dignity, suavity and good humour. The strong position in which the Royal Society of Tropical Medicine and Hygiene, with a membership numbering over one thousand, now finds itself, is due in great measure to the original impetus it received from its first President.

CHAPTER IX

Manson's Mosquito-Malaria Theory

Prolegomena : The Theory enunciated : Ross's Earlier Work on the Theory

To a future historian, who should happen to forget the difficulty with which new ideas make their way even in the world of science, it may seem strange that when Laveran, in 1880, made known his discovery of an amœboid animalcule with characteristic black pigment—*Plasmodium* as it came to be named—in the blood of sufferers from malarial fever, the commensurate discovery of the way whereby these animalcules get into a man's blood and, having got there, issue out again to infect the blood of other men did not soon follow. He might argue that it ought to have been fairly well understood at the time that an internal parasite must perforce pass through a larval development of some sort in the outside world before it can infect a prospective host, and that Manson had already demonstrated how in the case of an analogous parasite, *Filaria bancrofti*, the necessary larval development was effected in the body of a mosquito ; and therefore he might wonder why a number of eager observers, with Manson foremost, did not at once search for infective developmental forms of the malaria parasite also in that or some other common and ubiquitous bloodsucking insect. He might argue, furthermore, that long before the discovery of the

plasmodium, mosquitoes had lain under suspicion of being concerned in some way with periodic fevers, and also that, quite independent of that discovery, a sagacious American observer, A. F. A. King, had (in 1883) marshalled a mass of circumstantial evidence indicting mosquitoes as accessories in the causation of malarial fevers and had thus propounded a mosquito-malaria doctrine—hardly to be compared with a demonstrable scientific hypothesis—in complete harmony with Manson's established hypothesis of filaria transmission.

Our imaginary historian, however, should reflect that after the discovery of the plasmodium a legitimate interval must elapse before it would be generally acknowledged as a necessary concomitant of malarial fevers and be generally received into the assemblage of true parasites, since general agreement on these points would imply prolonged observation of its life-history and behaviour in the blood of man.

In the course of five or six years this part of its life-history had been traced, largely by Italian observers, and chief among them Golgi. It had been discovered that there are three common species of Plasmodium infecting man, each with its own distinctions of form and behaviour, and each directly responsible for a distinct type of fever, but one in their general manner of life: that is to say, that they live and grow inside the red blood-corpuscles, and that when, at a definite period, an individual parasite has attained its full growth, it breaks up bodily into a swarm of young forms which burst through the ghost of the corpuscle and are rained into the blood-stream, where each young particle (*merozoite*) may find a fresh blood-corpuscle wherein to repeat the story of its birth. This method of multiplication by simple segmentation or fragmentation is non-

sexual [1] and cannot therefore account for the perpetuation of the species.

It is convenient to mention here that it had also been observed of Laveran's parasite that some full-grown individuals do not break up, but remain entire and assume a sausage-like or crescent shape; and that when blood containing them is kept fluid during microscopic examination these "crescents" in the course of fifteen to twenty minutes acquire a spherical shape, and that some of the resulting "crescent spheres" then emit gesticulating filaments, which presently break loose. These motile filaments are known as "flagella," and their process of formation is termed "exflagellation." (The full meaning of exflagellation, which is not peculiar to the genus *Plasmodium*, was discovered in 1897 by MacCallum, in America, as will be explained in the appropriate sequel.)

The facts that the plasmodium lives upon the red blood-corpuscles and is adapted for regular growth and proliferation in the blood-stream are alone enough to stamp it as an endoparasite. But even when these facts had been known for seven years; even when Pfeiffer,[2] in 1892, from a study of the common *Coccidium* parasite of the rabbit's liver, had indicated the homologies between it and the malaria plasmodium, and had not only inferred that the latter, like the coccidium, must have some second method of proliferation outside the

[1] Non-sexual reproduction (which is quite distinct from parthenogenesis, or reproduction by the intact virgin) is very common not only among the Protozoa (in which group the plasmodium is included), but also among Sponges and Zoophytes; it occurs, too, in groups as high in the scale as Echinoderms and Ascidians. It does not supplant, but it alternates with, sexual reproduction.

[2] *See* a paper by C. M. Wenyon, *Lancet*, 1923, vol. ii., p. 68.

vertebrate host, but had also suggested the probable site and general nature of that proliferation, *in bloodsucking insects capable of transmitting the progeny in the act of biting*, not even then did anyone appear to grasp the idea of a mosquito-malaria theory that could be finally proved or disproved by the method adopted by Manson in the case of *Filaria bancrofti*.

Here, indeed, our imaginary historian, were he of the "progressive" liberal persuasion, might be amazed at the disinclination of pathologists and sanitarians to rush upon a path of discovery so inviting, though if he were of a happier conservative complexion he might be consoled with the reflection that the instinct, which man shares with the rest of creation, to stick to the beaten track is too well justified to need explanation. And the broad fact of experience—the beaten track—to which most men stuck, and as firmly as anywhere in the countries of the British Empire, is that malarial fevers are inseparably connected with undrained or "marshy" land at certain seasons, and are more or less curable by quinine. These things being so, the incurious man, thinking mainly of medical treatment and knowing that malarial infection, whatever it might be, is not carried direct from man to man by contact, was content with his quinine and had neither eyes nor ears for parasites; while the curious inquirer whose note was tuned to the parasite, still kept his eye on the "marsh," in the air or soil or water of which he imagined the germs of the parasites to be living free and lying in wait for man.

We conclude, therefore, that the reason why the very important interlude in the life of the plasmodium that is transacted outside the human body remained so long undiscovered is that none but a few specialists were interested, and that most of the specialists failed to

realize that the species of Plasmodium are true parasites specially adapted to an existence in living blood, and consequently that some further special adaptations, enabling them to make a safe escape from the blood of one host in order to ensure entry into the blood of another, are indispensable to their persistence as species.

To illustrate the slow advance of the parasite into the scientific forum, we may consider the history of the two stars, Manson and Ross, who at last illuminated the secrets of its life outside man's body. Manson, in China, firmly impressed, as we have noticed him to have been, with the idea of a material pathogenous entity of some sort in malarial fevers, did not even hear of Laveran's discovery until about five years after it had been proclaimed. Even then he searched for the parasite in vain, and it was not until he joined the Seamen's Hospital at Greenwich in 1892 that he was gratified by sight of it. In India, too, as late as 1893, the very existence of a parasite was the subject of dispute. Thus Ross, writing in the *Indian Medical Gazette* of that year, speaks in one place of the " supposed hæmatozoon," and in another place gives an array of reasons approving the view that the supposed parasites are the outcome of post-mortem changes in normal elements of the blood. It is not a reproach to these great men—still less to their contemporaries—that the parasites remained to them so long in obscurity. To generations educated before all the facts about normal blood-cells and all the methods of differential blood-staining were known, the malaria parasite might well remain a chimera.

Manson may have been late in learning of the parasite, but when he did get abreast of the subject and began to observe the living creature itself in the act of " exflagella-

tion," he saw like a prophet where its secret doings outside the body should be looked for and how the first steps of the search should be made. Naturally he recognized an analogy with *Filaria bancrofti*, whose "extracorporeal" development in a mosquito he had discovered in Amoy fifteen years before. Others, including Laveran, had noticed this analogy, of which the significance had been enhanced by the discovery in 1893, by Smith and Kilborne, that the protozoon blood-parasite of Texas fever of cattle (which in the cow's blood is propagated non-sexually by fission) is somehow propagated outside the cow's body, in cattle-ticks. But Manson saw a good deal more than this, and to appreciate the quality of his vision we must go back to the then mysterious phenomenon of "exflagellation."

The flagella thrown off by some of the spherical entire parasites, when blood is taken from a malaria patient, had evoked much discussion. Most observers explained the process as one of degeneration: the blood, they said, being now dead and cold, the parasites degenerate and die. This opinion harmonized with the view that the plasmodia were outsiders from the marsh and merely inconvenient trespassers in man. Manson, however, was one of a minority who understood that the activity of the flagella was a sign of life, and he was singular among this minority in divining that *since the flagella appear only when malarial blood gets outside the body, their purpose must be to continue the life of the parasite in the outer world.* His sage inference, therefore, was that the individual flagellum must be in some respects analogous (but not homologous) with the embryo of *Filaria bancrofti*—a vital particle by which infection is passed from man to the mosquito. By this further analogy the argument pointing to the mosquito as the necessary intermediary

in spreading the infection of malaria among men gained so much in force and point as to become worthy of vindication by methods of experiment such as he himself had pursued in following the " extracorporeal " history of *Filaria bancrofti*.

This, then, was the gist of Manson's mosquito-malaria hypothesis : that if a mosquito were allowed to gorge upon a person whose blood contained forms of the malaria-parasite capable of emitting flagella, the flagella would soon afterwards be found alive in the insect's stomach ; and that if the flagella were traced thence into the insect's tissues they would be observed to give origin to forms of the parasite capable in some way or other of infecting a human being. As a working hypothesis its definition was improved, out of Manson's own Filaria experience, by the postulates that the flagella must not be expected to develop in all species of mosquitoes, and may be expected to assume misleading disguises in the course of their development in the insect. It was thus good enough for any skilled investigator, since it gave him trustworthy bearings for the one clear course to be followed. But although its distinctive and pre-eminent value as a working hypothesis is beyond dispute, it is as well to consider now—lest too much be claimed for it—first, that one of its premises was not quite exact, though not at all misleading ; and, secondly, that it gave the direct clue only to the manner in which the indispensable intermediary mosquito acquires infection *from* man, not the clue to the way in which the infected insect passes on its infection *to* man.

To consider the first point. It has been noticed in the prolegomena that when malarial blood taken from a patient at the proper moment, and not allowed to

get dry, is examined microscopically, certain "entire" forms of the parasite are seen, in the course of fifteen to twenty minutes, to "exflagellate," while certain somewhat similar "entire" forms do not. In 1897, three years after Manson formulated his hypothesis, MacCallum, in America, working with a blood-parasite of birds (*Hæmoproteus*), akin to the plasmodium, made the exquisite discoveries which are finally expressed in the proposition that the individuals that do *not* "exflagellate" are female elements, analogous to, if not strictly homologous with, the ova of higher animals; and that the individuals that *do* "exflagellate" are the corresponding male elements, their flagella being similarly comparable with the spermatozoa of higher animals; and, finally, that the process by which the parasite multiplies outside of its vertebrate host, and is maintained and distributed as a species, is truly sexual and starts with the fertilization of a *female gamete* by a *male gamete*. Thus Manson's concept of the flagellum, though true enough in direction, was excessive in range—the flagellum is only the male parent of the forms of the parasite that infect the mosquito.

To consider the second point: Manson's hypothesis was in part based on his own observations of the larval development of *Filaria bancrofti* in the mosquito, and its implications, therefore, beyond that point were merely speculations vitiated by insufficient knowledge of the habits of mosquitoes; so that in the one case as in the other the manner in which the infected mosquito transmits its infection to man was left open for independent investigation.

Having formulated his hypothesis with great precision, why did not Manson, it has been suggested, undertake the experimental proof or disproof himself, in England?

If this question had not been suggested we should not have thought it profitable to broach the subject, since it seems obvious that the transmission of malaria should be studied in a country where the material necessary for experiment is obtrusively abundant—in a country, like India, where, in serene indifference to sanitation, malaria-infected communities live in pious and placid amity with a Natural Creation that includes swarms of house-haunting malaria-infectible mosquitoes, and a country also where prevalent meteorological conditions facilitate experimentation even though they may discomfort a sensitive investigator. If an investigator in England, starting in the darkness of 1894, could have reckoned on a steady supply of persons infected with malaria; if he could have foreknown that in this country the species of mosquito susceptible to malaria-infection are nowadays to be looked for not in the abodes of men but in the cowsheds and stables of the countryside; and if he could have foreseen that in this country these species develop the infection with the necessary certainty only under atmospheric conditions that must be ascertained and artificially maintained; if, in short, he had known a few of the things that we have learnt now, he would even then have been hardly on a level, as regards natural facilities, with a contemporary starter in the tropics.

As a matter of fact, Manson was anxious to attest his theory in a propitious country—in Demerara. But although, as we have seen, the British Medical Association actually promised him a small grant for this purpose, what was wanted, even more than money, by a private person bent on experimental work among a superstitious coloured population, was the official recognition that would be accorded to a delegate of the Royal Society, and if he could have got this

imprimatur he probably would have done what most men of his age and achievements would have been content to leave to younger men who had still to win their spurs, and would have gone to Demerara himself.

A story, told by Manson, of the days when the transmission of malaria by mosquitoes was still regarded as a freak-hypothesis, shows how his very enthusiasm may have tended to frustrate his prospects of official backing. One day when walking down St. James's Street there emerged from a club two well-known physicians who had been criticizing his mosquito work in the medical press. As they passed him one nudged the other and asked who the rather vigorous and florid gentleman who had just nodded to them might be. The other answered, "*Mosquito Manson*," at the same time significantly tapping his forehead with his finger. Manson turned and reciprocated the significant dumb-show. We know also that at that time there were wiseacres in the West who did not understand the value of a working hypothesis based on relevant fact, and expressed small respect for people "guided by the divining-rod of a preconceived idea."

In the end, like the Sower of the parable, Manson went forth to sow. He published his hypothesis, and he also explained and demonstrated the point of it to the younger medical men who now were beginning to consult him as a sort of oracle when they came home from overseas on leave. We know of men both from the East and from the West who received the seed, and one of them has admitted that it fell on stony ground where, although there was joy in receiving it, there was no deepness of intuition to afford it a root.

It is as well here to try to realize the task set before the one man, of the few that received the seed with joy,

who provided it with depth of soil wherein it germinated and brought forth fruit an hundredfold. The hypothesis postulated a mosquito, but the analogy forbade the assumption that it would be every kind of mosquito: the right species, therefore, unless it were hit upon by a happy chance, might have to be discovered by a process of repeated experiment which, since nothing was known about the behaviour of the flagellum and the disguises that it might assume in a mosquito, would involve not only persistent and exacting toil in defiance of disappointment, but also an uncommon technical skill, and the nicest and most critical observation.

Among the younger men who went to consult Manson in 1894 was Surgeon-Major Ronald Ross, I.M.S., who the year before had expressed doubts about the malaria parasite. Manson was out when Ross called, but his letter acknowledging the call might have been inspired by a doctor of divinity.

(No. 1.) MANSON TO RONALD ROSS

21 *Queen Anne Street, Cavendish Square,*
9th April, 1894.

DEAR SURGEON-MAJOR ROSS,—I am very sorry I was out when you called this afternoon.

I read your papers some time ago in the *Indian Medical Gazette* with much interest. As against —— and some others your position is perfectly correct. They are only physiological phenomena. But I am certain, judging from the minuteness and accuracy of your observations, that you have not seen the *Plasmodium malariæ*, otherwise you would never have failed to recognize its pathological character, and the reason you have not seen it is the technique you employ. It will give me great pleasure to be of any service to you, for I am quite sure you can do good work and have the patience to do it. The first forenoon (to-morrow, if possible) you have leisure, call on me and we can arrange to make the most of the malaria cases I have access to.—Yours very truly, PATRICK MANSON.

What followed the receipt of this letter was that Ross became a frequent and welcome guest at 21 Queen Anne Street; and the outcome of this hospitable reception by Manson is told by Ross in two articles in the *Indian Medical Gazette*,[1] in which he justified his earlier misconceptions of the malaria parasite. In the first of these, published in December, 1894, the following passage occurs: "During furlough in England, owing to the great kindness and instruction of such an authority as Dr. Patrick Manson, who has allowed me to study at Greenwich Hospital and elsewhere, I have been able to correct my views where they were wrong." In the second, published in October, 1896, is the following: "Going to England I submitted my precious articles, together with a number of photomicrographs, which had cost me about a year's labour, to Dr. Patrick Manson. He said the kindest things possible about my work but—showed me a malarial crescent. . . . I knew at once that here was something quite foreign to normal blood."

The ultimate result of Manson's friendly behaviour was that Ross not only went in with the few who received Manson's theory with joy, but became the one convinced adherent who voluntarily undertook the labour of demonstrating its fundamental truth and in the event gave it the perfect precision and added to it the final penetration that it lacked.

Ross's original professions of faith in the theory are to be found in an article on "Malaria and Mosquitoes," published in 1900 in *Nature*,[2] where, after a brief reference to the vanity of all previous conceptions of the life of

[1] *Indian Medical Gazette*, vol. xxix., 1894, p. 441, and vol. xxxi., 1896, p. 353.

[2] "Malaria and Mosquitoes," *Nature*, vol. lxi., pp. 522–7; 29 March, 1900.

the parasite outside the human body, they are stated in the following passages :—(1) " Such was the state of things up to the end of the year 1894. Speaking for myself, I can well remember the hopeless feelings with which I then regarded the problem. Fortune, however, was to be kinder to us than I had dared believe. At this very moment the key to the solution of the problem had already been indicated by Dr. Patrick Manson." (2) " It is true that he [Manson] endeavoured to predict the history of the parasites a little too far, and that he was in error [1] regarding the immediate nature of the motile filaments ; but the centre of his theory was invaluable. I have no hesitation in saying that it was Manson's theory, and no other, which actually solved the problem."

Ross's entry on the search for the occult parasite is mentioned in the same article, in the following passage : " I must now turn to my own researches. Dr. Manson told me of his theory at the end of 1894, and I then undertook to investigate the subject as far as possible." Manson had previously made the following mention of it in an article in the *British Medical Journal* of 21 March, 1896 : " Surgeon-Major Ronald Ross was in England last winter, and, being favourably impressed with the mosquito hypothesis, determined to work it out on his return to India. After much discussion we fixed on a certain line of experiment and observation which my experience in former years in Filaria work seemed to indicate as being likely to lead to some definite result one way or other." The enterprise was no doubt quite spontaneous on Ross's part, but the friendly co-operation of Manson is implied in the above passages and also in the fact that the two men were in correspondence during the four years over which Ross's researches extended.

[1] Not so much in error as out of focus.

But whether implied at the outset or not, Manson's tutelary influence was warmly acknowledged by Ross not only in his letters to Manson, in one of which he likens himself to the instrument directed by an astronomer, but also in his published statements. One of these, which appeared in 1898,[1] at the conclusion of his researches, runs as follows: "These observations prove the mosquito theory of malaria as expounded by Dr. Patrick Manson; and in conclusion I should add that I have constantly received the benefit of his advice during the inquiry. His brilliant induction so accurately indicated the true line of research that it has been my part merely to follow its direction." Another, which is to be found in his Nobel Medical Prize Lecture for 1902, on "Researches in Malaria" (Stockholm, 1904), states that "pour éviter tout commentaire erroné, qu'il me soit permis de déclarer ici que mes travaux ont été entièrement dirigés par Manson." And again, in his latest paper—a review of Wenyon's "Protozoology," published in the *Journal of Tropical Medicine and Hygiene* of 1 October, 1926—Ross explicitly states that "the life-cycle of the Plasmodidæ was discovered by Manson and myself," and "the priority belongs to us."

If Ross's undertaking was voluntary, he having his official preoccupations and responsibilities, equally was Manson's pilotage disinterested. The evidence for this statement will appear in some detail in the sequel; for the present we can merely anticipate it by three characteristic quotations from the subsequent public utterances in which Manson from time to time took pains to explain and extol the amplitude, the precision, and the originality of the discoveries made by Ross between the years 1895 and 1898. The first is from a speech made

[1] *Indian Medical Gazette*, vol. xxxiii., 1898, p. 451.

to the British Medical Association in 1898 and published in the second volume of the *Journal* of the Association of the same year. Here, after some descriptive particulars of the work, there comes the following appreciation of the worker :—

" I am sure you will agree with me that the medical world, I might even say humanity, is extremely indebted to Surgeon-Major Ross for what he has already done, and I am sure you will agree with me that every encouragement and assistance should be given to so hard-working, so intelligent, and so successful an investigator to continue his work. His observations tend to the conclusion that the malaria parasite is really a parasite of insects ; that it is only an accidental and occasional visitor to man ; that not all mosquitoes are capable of subserving it ; that particular species of malaria parasites demand particular species of mosquitoes ; that in this circumstance we have at least a partial explanation of the apparent vagaries of the distribution of the varieties of malaria. It seems to me that when the whole story has been completed, as it surely will be at no distant date, in virtue of the new knowledge thus acquired we will be able to indicate a prophylaxis for malaria of a practical character, and one which may enable the European to live in climates now rendered deadly by this pest."

The second personal tribute to Ross, extracted from the second (1900) edition of that well-known book, Manson's " Tropical Diseases," contains the following succinct but comprehensive account of Ross's discoveries (p. 21) :—

" Working on the lines suggested by this [the mosquito-malaria] hypothesis, Ross first (1895) demonstrated the fact that when crescent-containing blood is ingested by the mosquito a large proportion of the crescents rapidly proceed to exflagellation and to the shedding of the flagella. In 1897 (*Brit. Med. Journ.*, 18 December, 1897, *ibid.* 26 February, 1898) he showed that in particular species of mosquito fed on malarial blood, living and growing pigmented malaria parasites are to be found embedded in the stomach-wall of the insects. Early in 1898 (as announced

by me at the meeting of the British Medical Association, in Edinburgh, in July, 1898), he showed that if a particular species of mosquito be fed on the blood of proteosoma-infected birds, the parasite will enter the stomach-wall of the insect, grow and sporulate there, and that the resulting sporozoites will enter the venenosalivary gland of the insect, and that the insect will now be capable of infecting by its bite other birds. Ross further showed that only particular species of mosquito could subserve proteosoma in this way, and that the particular mosquito in question was not efficient as regards another blood parasite of birds—namely, halteridium—or as regards the malaria parasites of man. Thus by direct observation and by analogy Ross distinctly, and first, proved that the extracorporeal phase of the malaria parasite is passed in particular species of mosquitoes, and, by analogy, that the parasite is transferred from man to man by the mosquito."

Our last quotation is a happy remark made by Manson when vacating the Presidential Chair of the Society of Tropical Medicine in 1909, wherein he claimed credit for himself "not so much for having formulated the mosquito-malaria theory as for having discovered Ronald Ross." We owe this note to Major E. E. Austen, D.S.O., who recorded it at the time, being greatly impressed by its magnanimity.

To come back to beginnings: On his return to India in 1895 Ross started on his researches, following to begin with the exact method indicated by Manson and returning to that method after some temporary and ineffectual digressions—namely, the method of feeding mosquitoes on men whose blood contained what are now known as gametocytes and then progressively examining the mosquitoes for the results. It is hard to overestimate the sphinx-like difficulties of the task, seeing that the object to be looked for is infinitesimal and was then entirely inferential; moreover, there is hardly an animal, howsoever small, that

is not infested with parasites of its own, and in the then imperfectly known endoparasites specific to mosquitoes there lay infinite possibilities—nay, certainties—of mystification. Nor is it easy to overpraise the skill which—aided at a critical moment by a sudden flood of fortune—accomplished it, and not only accomplished it but put a beautiful and unforeseen finality to it; for neither Manson nor Ross had any idea at the outset—although the anonymous writer alluded to at p. 58, and the predictions of Pfeiffer (p. 123), could have supplied a hint of it—that a specific intermediary host could transmit a specific infection in the act of biting.

It is not our business to describe Ross's famous researches, but we must follow their course in order to show not merely how, in Ross's already quoted words, they "prove the mosquito theory of malaria as expounded by Dr. Patrick Manson," but how wisely and persistently Manson laboured to further them by counsel, by personal appeal to influential members of the Indian Government, and by keeping them and the name and fame of Ross constantly before the medical profession in England.

During the four years (1895–9) over which these researches extended Ross was in constant communication with Manson, reporting progress, describing observations and experiments, sending specimens for criticism, and in times of special difficulty appealing to Manson to use his influence in getting official bearings eased. The nature of Manson's response is shown in the following series of selections from his letters to Ross. In making these selections we have wished to illustrate their tone and spirit, which age cannot wither, rather than to insist upon the uniform infallibility of the advice. The advice undoubtedly was unequal—*neque semper tendit arcum*

Apollo. Where it was bent on the hypothesis and on the way in which a mosquito might acquire and incubate infection it could not have been more perspicacious, and even now is not staled; but where it went beyond the hypothesis, into ways in which the mosquito might *transmit* its infection, it was speculative and also invalidated by insufficient knowledge of the habits of mosquitoes. To a man working alone in an intellectual wilderness any intelligent advice and encouragement from afar is supremely refreshing: to have them from a wise man like Manson is indeed the gift of fortune. In truth at this point we cannot help contrasting the filaria discoveries of Manson in 1875–8 with the malaria discoveries of Ross in 1895–8, in the old, old light of the "Pilgrim's Progress": Manson, like Christian in that old story, had to fight his way alone; Ross, like Christiana, had a Mr. Greatheart always at hand and at help.

Manson's letters to Ross are, with two specified exceptions, addressed from 21 Queen Anne Street. In the first two (Nos. 2 and 3 of our series) Manson expresses his confidence in Ross and his satisfaction in Ross's discovery that the crescent forms of the plasmodium are not digested by the mosquito.

Ross had begun work by feeding mosquitoes on blood that contained crescents, and had observed that the crescents became spherical and that exflagellation occurred with particular freedom in the insect's stomach, which he therefore concluded was their natural nidus. He had also observed that 30 to 40 per cent. of the spheres remain quiescent and had concluded that these were on the road to death. (These are the forms that by MacCallum's discovery are now known to be female gametes.) In his experiments Ross seems to have used only such mosquitoes as he found at hand, namely,

species of *Culex* and *Stegomyia* (*see* his "Memoirs," pp. 156 and 201), in which, as he afterwards discovered, the Plasmodium of Man does not develop any further.

Disappointed in not finding in the stomach of these insects ("Memoirs," p. 156) any further visible signs of infection, Ross appears to have been drawn off at once into the subservient and speculative issues of Manson's hypothesis, in the endeavour to infect a man, assumed to be quite healthy, with a dose of water in which mosquitoes that previously had fed upon a malaria patient had died. By some unfortunate concurrence of events the experiment was supposed to have "come off" ("Memoirs," p. 156), and Ross communicated the misconstrued result to Manson, who in letter No. 4 of our series answered it with becoming caution.

Extracts from Letters No. 2–4. Manson to Ross

(No. 2.) 17*th May*, 1895.—I was delighted to get your letters from Malta and from Secunderabad and to gather from them that you pursue the plasmodium with enthusiasm. You do not lack for material and energy, and though you may get many a check I firmly believe you will work out some important result; you cannot fail to do so. Let this conviction fill your mind when inclined to despond, and look upon a stone wall as a stimulant rather than as an insuperable barrier to progress. I have run my head against them often enough, but somehow I kept hammering at it and thinking about it, and sooner or later I got over or under or through when I had set my mind to it, and so will you. . . .

Soon I expect to hear that you have followed the plasmodium into the mosquito or some other suctorial insect. If you succeed in this you will go up like a shot and get any facilities you may ask for in the way of leisure for investigation and laboratory appliances and assistance. I only wish I were you.

I got a guinea-worm case some time ago and have on hand a rare lot of infected *Cyclops*.

(No. 3.) *21st June*, 1895.—I am exceedingly obliged for your letters. I look forward to receiving them with the greatest interest, and when a mail passed without getting one the other day, I was terribly disappointed, for I thought you had fallen sick, or that you had got a check, or that you had given up the quest. Above everything, don't give it up. Look on it as a Holy Grail and yourself as Sir Galahad, and never give up the search, for be assured you are on the right track. The malaria germ does not go into the mosquito for nothing, for fun or for the confusion of the pathologist. It has no notion of a practical joke—germs are selfish brutes.

Your finds are most interesting and encouraging. The mere fact that the parasite is not digested is enough to prove to my mind that it is in its proper habitat, and the fact you have discovered that it advances in development more rapidly and more frequently in the mosquito's stomach is confirmatory of this inference. But now the struggle to advance the next step begins. . . .

I think you would do well to prepare a series of stained preparations from mosquito blood illustrative of what you have done. I think that such a series of preparations would show the progress of development better than anything else, as they could be made so rapidly at a few minutes' interval and could then be stained and examined at leisure. If you have time to do this and if you are generously inclined, send me a series either in bottle or stained, or both. I am appointed to open a debate or something of that sort on the malaria germ at the next meeting of the British Medical Association at the end of July, and if I were armed with a series of such preparations I could speak with an authority of your work which could not be controverted, and you may be sure I will give you a good shove and help you so far as I can to get the ear of the authorities in India who may be in a position to forward your investigations. . . .

About the next step. Among other things examine the dung, the little yellow specks you see on the glass the insect is kept in, for evidence of malarial products. It may contain nothing, but is easily examined. Then I should examine the ova of the mosquito from time to time before and after they are deposited in the water, for I have an idea that the beast gets into them and so into the larva and becomes a more or less permanent parasite, not only

in the individual mosquito that sucked him in, but in the progeny of this mosquito. This will be a more difficult matter. Then I should keep mosquitoes to maturity and tease them up in salt solution and examine their tissues one by one, searching inside all cells for something like the plasmodium. I question if he will be pigmented, but I am fairly sure that he is intracellular. This too will be a long and difficult task. But it is not without hope. Look about the kidney tube, or the analogous structure among the long cæcal tubes ; about the little corpuscles representing the blood-corpuscles in the walls of the stomach ; in the muscles of the thorax. Compare malaria-fed mosquitoes with non-malaria-fed mosquitoes and compare their progeny.

(No. 4.) *28th June*, 1895.—Your last letter was supremely interesting and, if the experiment you describe turns out to be something more than a coincidence, you may be congratulated on having clenched the whole matter of the rôle of the mosquito as alternative host of the malaria parasite. . . . Prove it by one or two more experiments and the Indian Government will give you anything you like in the shape of opportunity for further work . . . if they don't I shall take care that it is made pretty hot for them here.

I think I told you that I had to open a debate on the subject of malaria at the meeting of the British Medical Association at the end of July. Try to have me in possession of all your facts, and some slides if possible, up to date, and I shall make a carefully prepared statement of how things stand and bring the matter out, so to speak, officially. I think it would be a mistake to make any definite announcement yet or before we are quite certain. This much we might say even now, viz. that the crescent evolutes in the mosquito ; but that is not enough ; it is an interesting fact and a necessary fact for our theory, but it does not prove it. Your experiment on malariating a man with mosquito water does prove it, if repeated experiment gives similar results. . . .

Send specimens of the mosquito for identification of species. Possibly different species of mosquito modify the malaria germ ; that the differing degrees of virulence depend on the difference of species of mosquito that has served as an alternative host to the parasite. . . .

I have not had a malaria case for a month. I hear of some

mosquitoes at the Docks, and if I got my case I should have a try for the malariation of the insect and the consumption of his infusion myself.

For more than two years after his discovery of the maturation and liberation of flagella in the mosquito's stomach Ross failed to obtain any further light on the mechanism of infection in the mosquito. This, no doubt, was mainly because the press of important official duties left him but sparing leisure and infrequent opportunity for the study of the transmission of malaria. Moreover, he had taken up with one of the *ignes fatui* that then beset the path, and was led off into the study of a Gregarinid [1] parasite proper to the mosquito itself, in the conjecture that it might be a stage in the development of the malaria plasmodium ("Memoirs," p. 177). Furthermore, he attempted to infect himself and other volunteers with this parasite, and also made attempts, which his subsequent discoveries showed to have been doubly premature, to infect men with malaria by the bites of undetermined mosquitoes that had been fed on malaria patients. He seems, in short, to have lost confidence in Manson's theory that the flagellum was the only valid clue to follow.

We give here extracts of twenty-five letters that Manson wrote to Ross during these two years. In them Manson dissuades Ross from *a priori* attempts to infect men by the bites of mosquitoes; he gives Ross good reasons for concluding that the gregarine parasite of the mosquito can have no connexion with the plas-

[1] The Gregarinoids are an extensive Order of Protozoa of comparatively large size, parasitic in the digestive tract or the body-cavity of invertebrate animals and notoriously of insects and other Arthropoda. No Gregarine has ever been found in any true vertebrate animal.

modium of malaria, and although he admits that the study of this gregarine may have an educative value for Ross, he deprecates further study of such forms, as " blind groping " ; he recommends Ross to look in five- or six-day-old " malariated " mosquitoes for encysted bodies resembling crescents and to look for a sporulating stage in the mosquito ; he advises him to study the phenomena of exflagellation in the blood parasites of birds ; above all, he begs him again and again to stick to the flagellum. In his " Memoirs " (pp. 218–19) Ross has deprecated Manson's constant advice to follow the flagellum ; but to us the advice appears to have been the very plainest common sense at that early stage of the inquiry ; for an observer hanging on to the flagellum might, perhaps, have anticipated MacCallum's discovery of 1897, and so might have had a revelation of a young pigmentiferous zygote which would have enabled him thenceforth to distinguish, among all the objects that he might afterwards observe in a mosquito, the one sort that he was in search of. Ross, indeed, states in his " Memoirs " (p. 197) that he did unperceptively follow the flagellum to its destination ; that he did actually observe two pigmentiferous crescent-spheres in juxta-position, one of them disbursing flagella, the other remaining whole but containing a discrete flagellum, and that he did therefore *observe* the phenomenon after-wards *discovered* and interpreted by MacCallum ; but he also states that he missed and completely misunderstood the meaning of the phenomenon and ever afterwards felt disgraced as a man of science for his lack of vision. He attributes this misfortune—which certainly impeded him when he afterwards discovered the pigmentiferous cells in the dapple-winged mosquitoes fed on blood containing crescents—to his obsession with Manson's

conception of the flagellum as a spore. But in the enunciation of Manson's theory the flagellum was merely the *essential link*, the exact nature and mechanism of which had to be discovered by observation under experimental conditions; it was no part of the hypothesis—obviously it can be no bond in any working hypothesis—to foretell and accurately define all the subsidiary issues of the argument assumed.

With this second series of extracts from letters to Ross we also include a fine appeal made by Manson to Sir Charles Crosthwaite (the Permanent Under-Secretary of State at the India Office at that time) in behalf of Ross at a moment when Ross, dissatisfied with his prospects, had led Manson to fear that he might retire from the Service and relinquish the quest.

Extracts from Letters No. 5–29. Manson to Ross

(No. 5.) 10*th July*, 1895.—You will not get listened to at headquarters until your work has come out. I am preparing a manifesto for the British Medical Association Meeting, only some three weeks off, and I think that after this comes out you will be boomed perhaps more than you wish. . . . I have also some chance of making influence in your favour at the India Office. I know Sir Charles Crosthwaite and I am sure he would lend a hand if approached. I also know Sir A. Macdonnell, who I understand is a power in the State. I do not know him well, but I have seen him as a patient and expect to see him again soon. Him too I shall try to influence. Meanwhile work as hard as you can, and try to move the peg up one step higher. . . .

I can't say what these bodies[1] you find in the mosquito are. Very possibly, as you suggest, Coccidia. Might they be malaria? I should look in five- or six-day-old malariated mosquitoes for bodies like the crescent; for some body which is more or less encysted and prepared for a lying-in-wait period. I should also

[1] The "psorosperms" or spores of the mosquito Gregarinid.

look out for some intracellular form like the sporulating stage in the human body.

(No. 6.) *26th July*, 1895.—I am writing to-day to Cleghorn,[1] who is an old friend of mine, and I shall tell him all about the malaria-mosquito business and ask him to befriend you in the matter so far as lies in his power. . . .

There is not much doing in medical circles here just now. . . . You will rouse them, however, I am sure, and there will be a fresh spurt in malariology as soon as your work comes out. . . . I can see a big field of work springing out of yours. . . . I believe a splendid work for a man to spend his life over would be the influence of the mosquito as a pathological agent.

(No. 7.) *Littlewood Park, Alford, N.B. 22nd August*, 1895.—I hope to find your slides awaiting me when I return home. . . . I have permission to keep a " house " in the Orchid House at the Royal Botanic Society's Gardens, and mean to try to keep a stock [of mosquitoes] growing during winter, so as to avail myself of any good case that may turn up.

Here I have been on holiday since the 9th, shooting grouse and black game, and walking over awful hills ten hours a day. It does me a power of good.

(No. 8.) *9th September*, 1895.—The *Gregarina*[2] *culicidis* (*sic*) is interesting, but I look upon it as a good lesson merely, and not as the final outcome of your work. . . . I had a whole forenoon over your slides,[3] for which accept my very best thanks. I could make out the crescents in the mosquito very well, and I thought they were more numerous than in the finger blood. I could also see all the pigmented spheres in the older mosquito blood, spent spheres or perhaps flagellated bodies. They were quite distinct enough to convert me. . . . Next to the establishment of the fact that the mosquito's stomach is the most favourable place for the evolution of the flagellated body, the most important observation you have made is the discovery that the flagella can live three

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[1] Surgeon-General James Cleghorn, then Director-General of the Indian Medical Service.

[2] The Gregarinid proper to the mosquito.

[3] Of blood from mosquitoes.

hours at least as independent organisms, and that they attack cell structures.

Peg away; you are doing most valuable work, and I firmly believe that a light will dawn on you soon such as will astonish you and the rest of mankind and be a complete reward for all your efforts.

(No. 9.) *11th October*, 1895.—I am truly sorry you are off malaria and on to cholera. . . . Laveran is inclined to take up the mosquito hypothesis, and I have no doubt that by next summer the French and Italians will be working at it. So for goodness' sake hurry up and save the laurels for old England. It is in your power. There was an important meeting at the *Académie de Médecine* the other day discussing malaria prophylaxis in connexion with the Madagascar business, and Laveran was strong on the mosquito, so that I am sure the fat is now in the fire, and it is going to burn quickly. You have the advantage and can come in an easy first with a little luck.

The psorosperms are very interesting, not to say promising, but don't give up working on the old lines. Don't quit the thread. Follow up the flagellum. . . . I have spent a lot of time over the specimens you send me about the psorosperm ; I have an idea it is too big an organism in the mosquito to be related to that of man. . . . The psorosperms are also too distinctly capsulated to tally with the soft minute spores of the malaria bug.

Try to malarialize mosquitoes with ordinary tertian blood. Select the hot and sweating stage of the fever for feeding the mosquitoes. That is the time when the flagellate form abounds most in tertian blood. . . . Is it not curious as well as suggestive that this is just the time when the mosquito has the best chance —the patient is prostrated and cannot defend himself, very probably asleep; he has thrown his clothes off to cool himself, and his skin is bathed with perspiration, damp and attractive.

(No. 10.) *21st October*, 1895.—The long-looked-for consignment of mosquito slides arrived in good order . . . and I have had two long studies of the very interesting and important revelation of the facts they represent. They bear out all you have said about this beast [the mosquito *Gregarina*] and one can read the life-history of the little brute in your slides as in a book. . . . But, as I said

in my last, I do not think that this particular gregarine has anything to do with malaria. . . . You must go back to the flagellum and try to follow it up. But the gregarine lesson is not lost, for it shows how cunningly Nature provides for the spread of mosquito disease.

I got some mosquitoes, non-malariated, from Old Calabar the other day. . . . I found that, having been preserved in glycerine, the blood in the stomach had become so hard that I could not soften it and could not make anything out of the beasts at all. So I determined to get them sectioned and I send you by this post two slides to show you the result. I am sure you will be delighted with it, for it opens up a method by which one could easily follow up the malaria parasite in the insect and at the same time preserve a permanent record. . . . I would fain hope that by studying sections of the mosquito stomach in series we may be able to trace the progress of the flagella after they have broken away.

(No. 11.) *23rd December*, 1895.—I am looking forward to great things from your glycerine mosquitoes. I shall section them first; treat the hæmoglobin with weak acetic so as to dissolve it out, and then stain for the parasite. I believe I shall see the flagellated body in full form, and I believe I shall see the flagella on their way to the walls of the mosquito's stomach. Shall I be able to trace them further? If I can't, you must take it up in fresh insects, and if you fail with the microscope you must try what experiment will do. I have an idea that the fever state induced by the parasite is a condition invoked by the parasite in its own interest. . . . Nature is very dodgy. "Nothing walks with aimless feet."

(No. 12.) *29th January*, 1896.—The *British Medical Journal* sent me your paper on the parasites of Secunderabad and asked me to comment on it. This I have done. It will be published soon, I fancy, as there is rather a boom on in malaria lately in consequence of the death of Prince Henry of Battenberg. . . . Be therefore ready to strike now that the iron is getting to welding-point. I think you might publish categorically all your observations on Plasmodium in mosquitoes either here or in the *Indian Medical Gazette*, timing the publication so as to chime in with my [Goulstonian] Lectures. If the Government will move, then you are sure to be the man, and I do think that any reasonable application

for leisure to pursue the work, money, and opportunity will be given you. But you must follow up your flagella.

(No. 13.) *4th March*, 1896.—Very many thanks for the consignment of mosquitoes and blood-slide of mosquito blood. The latter shows the spherical bodies very well indeed, but I cannot as yet manage to bring out any flagellated form.

In my [Goulstonian] Lectures I make a point of suggesting to the Indian Government to encourage you and to give you opportunity, and I do hope they will act on the hint. I fancy they will. I shall also try to get the *British Medical Journal* to urge the same thing.

I am glad your cholera sanitation labours are near a close . . . and that you may have some time for higher things. If you get the mosquito business just one step further you are a made man.

(No. 14.) *18th March*, 1896.—I am looking forward with interest to the continuation of your paper in the *Indian Medical Gazette*.

I am anxious to get some good cases [of malaria] this summer to try the English mosquito on. I have a cage all ready for breeding purposes, but want the temperature, the patient, and the mosquitoes—in fact everything except the will.

I write now specially to tell you that I had a letter last mail from Hankin[1]—Agra—telling me that he was likely to have a man told off to him to work on the mosquito theory. I hope you are the man. . . . In my last [Goulstonian] lecture I detail your work and . . . mildly hint that men like yourself should have every opportunity given them to carry on such investigations. I hope they will take the hint and push you. But, if they don't, for goodness' sake don't let anyone else take the silver spoon out of your mouth. Hammer away as hard as you can and get ahead of every one else. . . . Above all, run the flagellum into its appropriate cell. Had I the chance I would work both day and night at this.

(No. 15.) *20th March*, 1896.—Just a line to say that your mosquitoes have so far turned out first-rate. I have only done one of them yet, but this one showed, after proper preparation,

[1] Bacteriologist to the Government of the N.-W. Provinces of Bengal.

dozens of the pigmented sphere, *and a suspicion of flagella in one or two of them*. So that . . . as we work our way through the lot . . . perhaps we may be able to follow this up into my hypothetical cell. [*The italics are ours.*]

(No. 16.) 18*th April*, 1896.—I congratulate you on the completion of your sanitary labours. . . . I am very much interested in your psorosperm experiments; go on with them, but I beg of you not to leave hold of the flagellum.

(No. 17.) 20*th May*, 1896.—I duly received your letter of the 20th of April. I gather from it that you are the victim of overwork, that you are suffering from despondency and exhaustion, but take heart; the wheel will turn again, and you will feel better by and by. You are quite right to take a holiday; it will give you time to think and time to forget. . . . Do not despair about your application to Cleghorn and Hankin.

(No. 18.) 12*th October*, 1896.—Bignami has criticized our theory in *Il Policlinico* and has started a mosquito theory of his own. I send you a translation of his article . . . prepared for the *Lancet*, in which it is likely to appear soon. . . .

Your experiments on mosquito-biting are interesting, although negative. It may be that the mosquito conveys the parasite in biting, but I do not think so—at all events I do not think that it does so directly; for the habit of the mosquito is to bite once only, and the normal evolution of the parasite is sure to correspond and to run parallel with the normal habits of the insect. . . . But I would reiterate the advice I always harp on—Follow the flagellum. Next case I get of crescents I mean to try to demonstrate nuclear elements in the flagellum. . . . Were I in a hot place I would try to work this point out in birds rather than in man, and I am not so sure that you might not be able to get along in this way yourself, seeing that there would be no lack of material and opportunity and you could get native assistants to work for you. You could easily get some bird with flagellated forms in its blood, stick it in a cage and the cage in a mosquito-house and set mosquitoes to bite, and have no worry about searching for crescents, squaring the man, accusations of homicide, and so forth. . . .

Write me whenever you get a chance. I like your letters . . .

and don't mind asking me to do anything I can for you, or anything to help forward the good work in England.

It is evident the Italians are now on the scent. I do hope you will run into the quarry before them. . . . Laveran is working up the Frenchmen. I do not hear that the Germans are moving, but they will, and so will the Russians.

(No. 19.) *27th November*, 1896.—Don't be cast down at difficulty and temporary failure. You won't fail if you stick to the flagellum. The more I see of this wonder and the more I think of it, the more I am convinced of its importance and that it is the extracorporeal spore of the plasmodium. I would be inclined to still continue watching the flagellum in the mosquito stomach; giving it the offer of different kinds of cells from the mosquito; trying to see if it attempts to enter some particular kind of cell. The epithelium of the stomach is the first cellular structure it is up against. Try it. It looks juicy and comfortable in some of my sections. When you have got a method for flagellum staining, then try sections of the mosquito at intervals from the time of feeding. Don't forget to search the ova of the mosquito. I shall see Lewis this afternoon about the article in the *Quarterly Journal of Microscopical Science* and try to get the number with the article on mosquito anatomy for you. I have been at a good insect man, but beyond the anatomy of the cockroach and of the house-fly there is very little known about the microscopical structure of insects. . . . If you send me mosquitoes I shall get them sectioned and carefully examine them. The last batch turned out very well, and the slides you sent me, with one exception, showed well. But I can't get further than the sphere, although *I think in one I found flagella.* [*The italics are ours.*]

(No. 20.) *25th December*, 1896.—Your letter of the 30th November interested me very much, more especially as I was working at the same subject as yourself, namely, exflagellation and the cause of it. But our conclusions—or rather results—are diametrically opposite, for I find that I get exflagellation by moistening my slides. The fact is that neither hydration nor dehydration of the blood has anything to do with it. Something else is at work, something that takes place no matter whether the blood is dense from evaporation or light from the addition of water. At the same time, exflagellation does not take place if the blood is

rapidly transferred to a perfectly clean and dry slide with a minimum of exposure to the air. . . . I have succeeded in staining the flagellated body. [He then describes the method, which he published at this time.]

(No. 21.) *21st January*, 1897.—I have no doubt that you will solve the problem yet, but you must be prepared for no end of work and many disappointments. For example, yesterday afternoon I spent in the hospital trying to get some crescents to exflagellate, and exposed the droplets "à la Ross" for 40 seconds and for 1 minute, and yet although nearly all the crescents became spheres, I did not get one flagellated body. The cause of exflagellation is evidently exceedingly difficult and complicated; however, your experiments prove that exposure sometimes brings about favourable conditions. . . . Go slowly about the eye-parasite. Have you stained it? If not, wait before publishing. To-day I saw a similar appearance in the blood in a case of septic infection from a rat-bite. Don't be seduced into going away to work at plague; there are plenty of men there already. If I were Surgeon-General Cleghorn, the first thing I would do would be to bring a *jehad* against the rats. . . . Plague has a feeble hold as an infective disease on a human community, and if the chances for the bacillus were to be diminished by only one tributary agent, such as the rat, I believe the disease would quickly die out.

(No. 22.) *3rd February*, 1897.—Your paper appeared in last week's *British Medical Journal*. The paper is a splendid one and will give a fresh spurt to the study of the flagellate forms. . . . You are right about [the flagella] being derived from the central mass, which grows visibly smaller as the flagella come off or out of it. On Monday I repeated your vaseline experiment and found that your description is correct. There is no exflagellation so far as I saw until the bleb of blood is broken up and exposed.

I have now some specimens of stained blood with free flagella but I cannot make out any nucleus and believe that the chromatin is diffused through the flagellum. . . . I have set my man on the mosquitoes again, sectioning as before, but using the slow staining with dilute carbol fuchsin. . . . I have promised him a fiver when he shows me a flagellum inside a gastric cell, and I shall have much pleasure in paying him the money.

(No. 23.) *20th April*, 1897.—I am very glad to hear that the plasmodium is to the fore again, and that you will be on his track so soon. Stick to him. Sooner or later he will do much more for you than sanitation work at Bangalore, although I should have liked for your immediate comfort's sake that you had been confirmed in your appointment there.

(No. 24.) *24th May*, 1897.—I have had my telegraphic address registered . . . and I hope the first message . . . will be from you to the effect that you have run the plasmodium to earth, and that it is a parasite of the mosquito. Be very sure of your facts before making the announcement; but if you are quite sure of your facts, don't hesitate to make it. You must be ahead of the Italians and the Americans.

(No. 25.) *4th July*, 1897.—I have been much interested in your last three or four letters, and am hoping that each one would add some clue to the problem you are trying to solve. This much they show, that the problem is much more complicated than at first sight one would think. And this they have also made me feel, that you won't get forrader unless you hang on to the flagellum. Stick to it, and try by all sorts of microscopical device to hang on to its tail and get, so to speak, dragged with it to its destiny. Working with these other forms is more or less blind groping; sticking to the flagellum is following the leader.

Surely some temporary disappointment has sent the evil thought to chuck the Service. . . . Surely your better angel has crushed this thought ere this. . . . You must be wrong in saying that there is no reward for such work as yours. Administrators and surgeons are thick as peas anywhere, but a man like yourself, who is capable of original work, of discovery, is not to be picked up every day, and no Government in its senses would ignore persistently your existence or fail sooner or later to reward your good work. For very obstinacy and determination not to be beat I would stick to my last were I in your place.

(No. 26.) *7th July*, 1897.—I have received yours of the 12th June. I immediately wrote to Sir Charles Crosthwaite. . . . He is a man of great influence at the India Office. . . . However he answers, don't do anything in a hurry. It is so easy to break a sheet of glass; so impossible to mend it. I don't believe that any Government can ignore your work.

(No. 27.) *22nd July*, 1897.—I was very glad to gather from your last that your chance of getting your heart's desire is now so good. I did not think that, unless the authorities were mad, they could overlook your claims, or, rather, their own interests. Sir C. Crosthwaite has interested himself a good deal about you. . . . The *British Medical Journal* had a paragraph about you in their last issue, which may be of some use. In the same number you will see the announcement of my appointment [as Medical Adviser] to the Colonial Office. This should be an encouragement to you to work on, believing that in time your reward will come. I had no influence whatever with the authorities and made none. . . . So that, you see, the casting of bread on the waters bears fruit, though it may be after many days. This appointment . . . will bring with it many opportunities, and if I can see my way I shall do my best to give a leg-up to the teaching and study of tropical medicine. . . . If I can get my way no man shall get a licence to practise tropical [medicine], at all events as a public servant, who can't demonstrate the malaria parasite and one or two others.

(No. 28.) *23rd July*, 1897.—I have just got a letter from Sir Charles Crosthwaite. He writes: "I have got the Secretary of State to send in your letter to me about Ross to Lord Elgin,[1] and hope it will bear fruit." I pitched it very strong about neglect of opportunities, of efficient and willing men, about the importance of the subject, and the backward position of England in this matter, etc., etc.

(No. 29.) *11th August*, 1897.—I had no time to write to you by last mail, but got my wife to copy out two most important papers which have recently appeared in France and which go a long way to support our view about the life-history of the malaria parasite. The writers do not seem to know about our particular view, but that makes no difference to the view or the value of their support. . . . The close correspondence with the spore phase and the rosette phase of the Coccidia is very remarkable. I am more convinced than ever that the mosquito is the alternative host, and I hope you will soon be able to prove it.

Some time ago I had a crescent case . . . in hospital, and again

[1] Viceroy and Governor-General of India at that time.

repeated your experiment with vaseline with complete success. At the same time I made a number of stained preparations of the flagellated body with excellent results. I have now many good stained specimens, and among them not a few free flagella both near and at a distance from the parental sphere. But I cannot make out a nucleolus. The free flagellum seems sharper at one end than at the other. Would it not be possible to watch the flagellum in a crushed mosquito's stomach and follow it up to its destiny in some stomach or other cell? I still think you have got to hang on to the tail of the flagellum to be put on the right track. Once find out the cell and you are all right to trace it to its final stages. I think you should name the various organisms for your own use, but not, as yet, for publication, as it is just possible that they may have been named already.

FROM MANSON TO SIR CHARLES CROSTHWAITE

5th July, 1897.

DEAR SIR CHARLES CROSTHWAITE,—You may be able to recall to your mind the mosquito theory of the malaria microbe, which I fathered some time ago. It would appear that this theory has excited some attention among foreign and English pathologists, and its reasonableness and probability have been generally recognized. More especially is this the case in America, Italy and France.

A considerable amount of support to the doctrine—which it is needless to say, if proved correct, will have far-reaching practical consequences—comes from Surgeon-Major Ronald Ross, I.M.S., at present stationed in the Madras Presidency, who by a series of laborious, elaborate and very delicate observations, has distinctly proved that the malaria microbe does enter and evolve to a certain point in the mosquito. Naturally, Ross is very anxious to go on with his work, but finding that his devotion is likely to hamper his prospects in life unless the Government see fit to afford him certain facilities and encouragements, he has asked me to try and make some interest on his behalf so as to enable him to conclude the work he has begun so well.

I have no influence, but I thought that perhaps you might be willing as well as able to help a big cause and a most deserving man, either by advice or by judicious representation in the proper quarter.

The enclosed correspondence and papers explain what has

transpired already, and what Ross's position is, and what he wants. Unless he is helped to a very modest extent, Ross, discouraged by the scant encouragement or even justice he has received, proposes to give up the Service altogether, take his pension, and fall back for a living on some literary work offering at home. This threat, if carried out, would, in my opinion, amount to a public calamity, for there is no one in India, so far as I am aware, who is so competent to carry on the proposed malaria investigation as Ross, and it would take years of work for any man to educate himself up to the point of special qualification which Ross by his long-sustained work, his devotion and his great natural ability, has attained to. What Ross says about the proposed Pathological Institute the Government of India has in contemplation, is quite correct. Bacteriology is only one department, and a man may be a good bacteriologist and yet be in complete ignorance of the class of organisms to which the malaria parasite belongs. Money can build institutes, but a thousand institutes are useless if they are not manned by the right men. A good man is worth them all. I have no hesitation in saying that at the present moment Ross is the best man in India to carry on malaria investigation. To lose him, therefore, as I say, would be a public calamity.

Could you do anything to help him, and through him the cause of pathology, or could you advise me how to set about influencing the Indian Government to supply and to comply with the very modest demands Ross brings forward as the condition of his remaining in India? Ross is a poor man, I believe; he has a family, and must make a living as well as provide for his family in case of his death.

So convinced is Ross of the truth of this mosquito theory that lately he took three months' leave and retired to the most unhealthy place he could get access to, to work on the mosquito. He was three months there and got a lot of knowledge, and also a dose of malaria. Considering that his family depend on him, he does not feel justified in perhaps sacrificing life even in so good a cause. I mention this to show you that Ross is no self-seeker, but a genuine devotee in the cause he has at heart, which to him is only second to the duty to his family.

It would be a vast pity if the chance which now presents of making a substantial addition to the pathological science should once again be lost to Englishmen. We are cutting a sorry figure

alongside other nations just at present. To our national shame, be it said that few, very few, of the wonderful advances in the science of the healing art which have signalized recent years, have been made by our countrymen. This is particularly apparent in the matter of tropical diseases, in which we should in virtue of our exceptional opportunities be *facile princeps*. But, even in tropical diseases, Frenchmen, Italians, Germans, Americans, and even Japanese, are shooting ahead of us. We have to get a Koch to find for us the cholera germ, and a Haffkine to protect us from it; a Laveran to teach us what malaria is, a Kitasato to show us the germ of plague, and a Yersin or a Haffkine to cure us of its effects. This is very humiliating to every one but a cynic.

But in this matter of malaria here is a chance for an Englishman to rehabilitate our national character and to point out to the rest of the world how to deal with the most important disease in the world—malaria. This is not bunkum, but fact. If we don't do it, and do it soon, some Italian or Frenchman or American will step in and show us how to do what we can't or won't do for ourselves. They are on the track even now. Can you influence the powers that be at the India Office to give Ross the chance of striking a good stroke for England? He asks for little, and may do a very great deal indeed.—Yours very truly, PATRICK MANSON.

CHAPTER X

Manson's Mosquito-Malaria Theory (concluded)
First Verification by Ross : Manson's Later Letters to Ross : Manson's Practical Demonstrations of Malaria Transmission

In course of time the importunity of official work relaxed, fair leisure and opportunity for the study of malaria-transmission seemed assured, and Ross's faith in Manson's hypothesis appears to have revived; for on 27 July, 1897, he wrote to Manson expressing his full appreciation of the view that the process of exflagellation must be an adaptation to the mosquito, and announcing that, since he had got some good cases of malaria with crescents in the blood, he intended to compare mosquitoes that had been fed on these patients with mosquitoes that had not touched malarial blood—to examine their tissues and their excrements and so to find out whether the flagella grow in the mosquito. In short, he proposed to do what Manson had advised in discussing the " next step " in his letter to Ross of 21 June, 1895 (p. 140), namely, to " examine the dung," to " examine the tissues one by one, searching inside all cells for something like the Plasmodium," to " compare malaria-fed mosquitoes with non-malaria-fed mosquitoes "—in sum, to return again to the strait gate and to the narrow way.

Proceeding in this sound way Ross failed to find anything in the " grey " and the " brindled " mosquitoes

with which he again began to work, and therefore came to the conclusion that the flagellum does not continue in these species. Then, by a turn in Fortune's wheel, some large brown mosquitoes with dappled wings fell into his hands. Ten of these, clean and fresh from the pupal case, were given their virgin feed on a man carrying crescents in his blood; some of them died, but in successive dissections of the survivors Ross discovered in the stomach-wall of two individuals, on the fourth and the fifth respective days after their feed, certain entirely novel cells containing the black pigment characteristic of the small Plasmodium group of animalcules. The anticipated discovery was thus laid open, and Manson's hypothesis of malaria-transmission by mosquitoes *as intermediate hosts* of malaria parasites was verified—and verified through the same kindness of Fortune as Manson (though at an earlier stage) had enjoyed in demonstrating his theory of Filaria transmission, namely, a fortunate encounter with the right species of mosquitoes for experimentation.

The facts that the novel cells in the mosquito-stomach contained the characteristic malarial pigment, and that the mosquitoes had fed on nothing but malarial blood, were valid evidence of the reality of Ross's great discovery, even although Ross, having failed to follow—or, rather, to interpret—the first move of the flagellum ("Memoirs," p. 197), could not then fully and conclusively account for the presence of the pigment in those impressive cells; the facts were quite sufficient proof for a preliminary announcement giving Ross incontestable priority of achievement. To express the facts in the familiar terms of to-day, Ross had discovered, in the stomach-wall of mosquitoes of the particular genus *Anopheles*, fed exclusively upon a man infected with

Plate X

A DIAGRAM OF PHASES OF THE MALARIA PARASITE

Figs. A–F and the corresponding diagrammatic figures 1–7 are drawn from Manson's original specimens (1897), and illustrate the development, in chilled human blood, of the "flagellated body" (Fig. 6) from the crescent (Fig. 1), and liberation of the flagellum from the sphere (Fig. 7). (These stages were determined as the result of his original observations, upon which the mosquito-malaria hypothesis was founded.) Figs. 8–11 illustrate the stages determined by MacCallum (*see* p. 128), Figs. 12–19 the cycle in the stomach-cells of the mosquito discovered by Ross and subsequently confirmed by Grassi and his colleagues.

Fig.

1. Crescent or male gametocyte of the malaria parasite in human red corpuscle.
2–4. Male gametocyte becomes transformed into a sphere.
5. Formation of flagella or microgametes within the sphere.
6. Exflagellation, or extrusion of microgametes (flagella) from the sphere.
7. Liberation of free microgamete, or flagellum, from the sphere.
8, 9. Fertilization of female cell, or macrogamete, by the flagellum (microgamete) in the mosquito's stomach.
10. Fertilized female cell or zygote.
11–13. Transformation into the oökinete, or travelling vermicule.
14–17. Development of the oöcyst from the oökinete between the epithelium and the elastic membrane of the mosquito's stomach.
18, 19. Formation of sporozoites, or "germinal rods" of Ross, within the oöcyst and their migration to the salivary gland of the mosquito.

(*The lower portion of this plate is based upon Fig.* 392 *in Wenyon's "Protozoology," by permission of the publishers, Messrs. Baillière, Tindall & Cox.*)

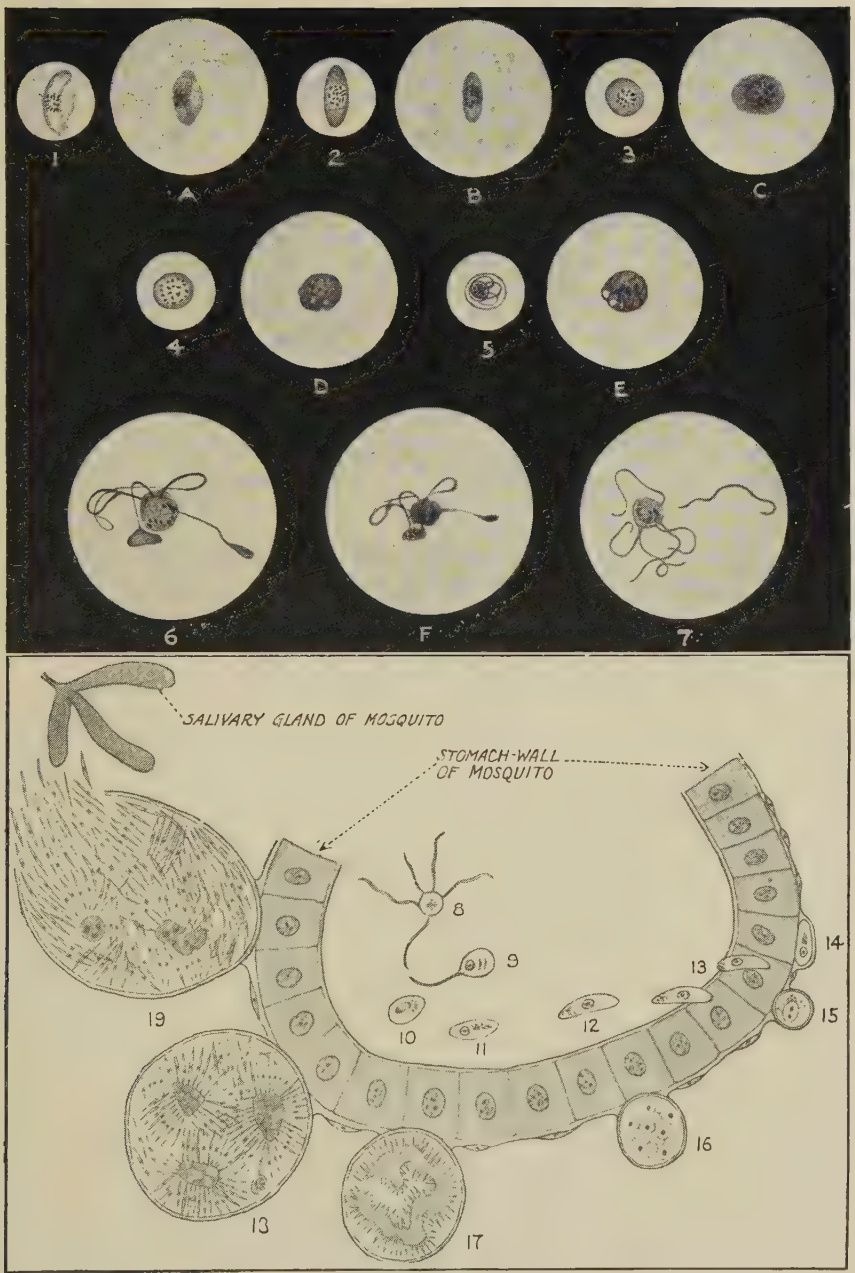

PLATE X.—A diagram of phases of the malaria parasite.

(The lower portion of this plate is based upon Fig. 392 in Wenyon's "Protozoology," by permission of the publishers, Messrs. Baillière, Tindall & Cox.)

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malaria parasites, the developing oöcysts generated in the sexual cycle of the parasites. Those terms, however, had then still to be disclosed and defined, and the first steps in this further process of discovery were to be illuminated by MacCallum, who, in Manson's phrase, "followed the flagellum" in the *Hæmoproteus* of birds.

That Ross did not immediately clinch his discovery against all the assaults of pragmatical critics is another illustration of the fickleness of Fortune. The full flood of his success was for some months set back and overwhelmed in a sea of Departmental troubles. He was one of a good many officers of the Indian Medical Service whose private concerns—owing to the temporary depletion of the Service—had to be thrust aside in the departmental exigencies of the Tirah Expedition of 1897–8. All officers of the Service, no matter where they were or what they were doing, were "mobilized" for military duty, whether they were ultimately required in the field or not. So far as opportunities for distinguishing himself either on active service or by scientific research went, Ross found himself paralysed. He feared that he would have to abandon his malaria investigations altogether, but he first appealed to Manson to exercise all his influence in enlisting the interest of the Indian Government in the mosquito-malaria hypothesis. It was in this direction that Manson was still able to render, and did render, valuable assistance, now that the essential truth of the hypothesis was confirmed, in Ross's own independent consummation of the hypothesis.

The following extracts from twenty-five letters to Ross show how constantly and magnanimously, for another sixteen months, Manson stood by Ross, and how diligently and in how many ways he sought and served

Ross's interests and publicly defended his rights of priority (*see* pp. 162, 163, 164, 170, 171, 173, *also* p. 135).

Manson not only busied himself among personal friends and others who had influence in the India Office or with the Indian Government, in order to get Ross placed on special duty; but also he took trouble to get Lord Lister (then President of the Royal Society) and Joseph Chamberlain (then Colonial Secretary) interested in Ross's discoveries, with a view to Ross's future advantage. It is evident that it was very much due to Manson's persistent advocacy that Ross eventually obtained the special appointment in Calcutta where, working with the common house-mosquito (*Culex fatigans*) and the Plasmodium (or Proteosoma) of bird-malaria, he followed, with an accuracy and a final completeness that time has not touched, the sexual cycle of the Plasmodium in the intermediary mosquito, and also made the magnificent discovery—all his own—of the living mechanism whereby the infected intermediary transmits the infection to Vertebrate hosts in its saliva, in the act of biting.

Along with this last series of Manson's letters to Ross may be included a letter to Manson from Surgeon-General Cleghorn, another from Lord Lister, and another from Surgeon-General Harvey—all of them indicative of Manson's solicitude for Ross.

EXTRACTS FROM LETTERS No. 30–54. MANSON TO ROSS

(No. 30.) *1st October*, 1897.—I put off replying to your last two letters hoping that the next mail would bring some fresh news about those most interesting pigmented cells.[1] The *B.M.J.* sent me your paper asking me for my opinion about its publication. I told them it should be published at once, partly because

[1] I.e. the "oöcysts" discovered in the dapple-wing mosquitoes.

we want to secure priority for English discovery and partly because it will stimulate others to work on the same lines. It will do you good, too, by showing that you are not lying on your oars. The thing is really most promising. If you get another opportunity, try the chemical tests for the pigment. It is a pity you did not do that at the time ; I can quite understand, however, your disinclination to spoil the specimens. There is just one thought that occurs to me. Is it not possible for the stomach cell to include pigment from pigment in the blood in the contents of the stomach? I fancy some doubting Thomas may raise this point, and it would be well for you to notice it so as to show that you are alive to all objections and that you do not fail to weigh them.

You must get more of these mosquitoes and go over and over the ground a dozen times to be sure that the step is a genuine one. If it is genuine, the problem seems to be practically solved, for the rest is merely a matter of work and detail. You know where to go for your material now.

I spoke again to Hooper [1] about you. It seems that Cunningham of Calcutta is likely to retire soon. His billet would be just the place for you, and I fancy that Hooper, if he has any say in the matter, will say it in your favour. I suppose the emoluments are sufficient. I am sure the work would be to your mind.

(No. 31.) 21*st October*, 1897.—You seem to be on it at last. The slides arrived a fortnight ago and while I was away in Scotland on my week's holiday. I examined them at once on my return and found them in good condition enough to recognize the cells you describe.[2] But the preservative medium was evaporating, and, fearing the cells might perish, I had them drawn at once by a good artist, and I hope they will appear soon in the *B.M.J.* along with your paper. I showed them to Thin and to Bland-Sutton, who at once recognized their likeness to the malaria parasite and the importance of your find. Personally I believe you are on the road to great discovery, but first you must be sure of where you are. Two things occur to me. The pigment in the cells

[1] Surgeon-General Sir William Hooper, I.M.S., President of the Medical Board of the India Office, 1895–1903.

[2] Zygotes of Plasmodium.

may be absorbed by these bodies from free pigment in the mosquito's stomach; second the pigment may represent an entire malaria parasite which has been bodily incorporated from the blood by the stomach cell. In either case it would not represent a phase in malaria evolution, of course. These possibilities you must try to get rid of if you would choke off carpers. I asked the Editor of the *B.M.J.* to request Thin and Bland-Sutton to make a brief statement to the effect that they had seen your slides and that they consider your descriptions were correct. A similar statement I have made myself, and I suppose these corroborative statements will appear as an appendix to your paper. I told them to hurry up with the publication.

I hope your new appointment will suit you in every respect. I have been urging your claims in all directions, and I hope there may be some good come of it. . . . Meanwhile I enclose a letter sent me by Sir Charles Crosthwaite, which will show you that you are not being forgotten. If you would only make one step further in malaria you are safe. . . .

You will have seen from the medical papers that I have been rubbing it in again about medical education *re* tropical subjects. That has bruised a good many toes, and has made me enemies as well as many friends. I have only spoken the truth, and they can revile as much as they like. My colleagues, or at least one of them, at the hospital are apparently very wroth about it.

(No. 32.) 17*th November*, 1897.—Probably this week's *Journal* will contain your article, and you will see from it what I have done to shove the matter on. Your position has been well guarded, and there, in future, can be no question in the matter of priority in discovery. I also wrote the leader, so as to give the friends you have a handle to work with in attempts on the Indian Government. I have ordered a number of reprints of article and leader. Some of these I will send to you; some I will keep for distribution here. Don't despair. If I can work it I shall not lose an opportunity of doing so.

You will see in last week's *Lancet* a short notice by MacCallum, of John Hopkins Hospital, about a new view on the meaning of exflagellation. It may be correct; but even if correct it does not invalidate the mosquito theory. Perhaps fecundation of the sphere takes place in the mosquito's stomach prior to the forma-

tion of the vermicule, which is really the penetrating spore carrying the plasmodium into the mosquito-cell.

(No. 33.) *27th December*, 1897.—I duly received your telegram of the 3rd inst., and letters of the 6th and 9th December. . . . The telegram came first, and I at once set to work to find out what could be done. I wrote to Sir Charles Crosthwaite enclosing your article in the *B.M.J.* and the leading article thereon. I enclose his reply. Of course the latter is very private and confidential, and not to be used. I also went to Sir Joseph Fayrer [1] and had a long talk with him. I also consulted the *B.M.J.*

It is evident from Crosthwaite's letter that he does not think it advisable to force the hand of the Indian Government, and Fayrer is of exactly the same opinion. . . . Fayrer was very decided about this. By Fayrer's advice I have written Cleghorn, who is an old friend of mine, and have asked him plump to lend you a hand. Fayrer has done the same. . . . There the matter rests at present. The article in the *B.M.J.* was a very strong one, and if Cleghorn is to be influenced by public opinion it should do the trick for you. The notes by Thin, Bland-Sutton and myself appended to your article will show Cleghorn what people here think of your work.

However things go yet awhile, don't throw up the sponge. Of course the hardship you are called on to endure is very galling and almost insupportable; but such is the fate of nearly every one who makes a new departure, and much of the merit of the discoverer lies in his faith and perseverance under difficulty. There are very few things worth having that are easily got. . . . I feel that if you carry this mosquito business one step further, that the battle is won and that your future is assured. I feel that if you chucked it now you would regret the step ever afterwards, for some one else would enter in and reap the harvest you have prepared with so much labour. If you succeed in proving the mosquito theory and if then the Indian Government will do nothing for you, such a howl will go up from this side that they will shake in their shoes and have to mend their ways sharp. . . .

Wait, therefore, till Cleghorn has got Fayrer's letter and mine. I think he will do his best for you. If you get a suitable place,

[1] Surgeon-General Sir Joseph Fayrer, Bart., K.C.S.I., F.R.S., President of the Medical Board of the India Office, 1875–95.

then work as hard as you can so as to justify his action, prove the theory and you will establish claims which will be irresistible. I think that you will find that Cleghorn will befriend you.

I hope that your next letter will tell me that you have had a transfer to some suitable spot and that work is in full swing again. The interruption was most unfortunate, coming as it did when you seemed just on the point of laying your hand on the prize. . . . Much detail will have to be filled in afterwards, particularly the study of the various malaria-bearing mosquitoes and the relationship of particular species to particular parasites. I fancy some of this might be better done in countries where special types of parasite are exclusively present. It would be easy to find out if there is a special mosquito in quartan lands, in tertian lands, and so on. The mosquito species have all to be collected, classified and separately studied in relation to the parasite. Wishing you patience, a fixed purpose and good luck.

(No. 34.) *7th February*, 1898.—Yours of the 19th Jan. by last mail. I had also a letter from Cleghorn. From the latter I judge that Cleghorn is doing all he can for you. He writes :—" Ross sends me a copy of all his papers and I have read them with great interest. Some time ago I strongly recommended that he be placed under my orders, with a good salary, so that he might be enabled to carry out his investigations without let or hindrance. The proposal would have been agreed to under different circumstances, but financial difficulties and the paucity of medical officers prevented its being carried out. . . . When the medical officers are returned from military duty on the frontier and the present strain is relaxed, something can then be done. . . . Your letter to Sir C. Crosthwaite was submitted to G. of I. and they were quite prepared to do what they can, but it is impossible to do anything now."

It is evident from the foregoing that you have a friend in Cleghorn, and that when the war is over and the plague subsides that your turn will come. It is not nice to have to preach patience, but it is the only sermon which, in the face of Cleghorn's very frank letter, can be preached. I feel sure that you are not being ignored, and that when the time comes you will shoot ahead. Meanwhile do the best you can with the poor opportunities at your command. If you cannot get malaria you can get crows and

pigeons and frogs and other coccidium-afflicted creatures to work on. Much can be learned from analogy and comparative pathology, and I am not sure that you could not do a very great deal, especially in the experimental line, by working on the lower animals. Don't forget that mosquitoes bite birds and that the plasmodia of birds are just as likely to be conveyed through mosquitoes as are the plasmodia of man. You should go over MacCallum's observations on Halteridium and see if it is correct, for, if correct, it is of the very greatest importance. If the *polymitus* [1] is a fertilizing factor in the halteridium cycle, then the flagellated body of malaria is also a fertilizing factor in the plasmodium cycle. And if this is the case, and the fertilized crescent-derived sphere becomes transformed into a travelling cell-piercing vermicule in the stomach of the mosquito, we have the explanation of the pigment in your mosquito stomach cells; they are carried into the cell by the vermicule. I assume you have read MacCallum's paper in the *Lancet*. This is a way of reconciling your observations and his; the one set of facts explains the other. . . . I believe with you that you have found the mosquito phase in these cells, but, to prove it; there's the rub. Think of the pigment in connexion with MacCallum's vermicule, and try if you can find in the stomach of the mosquito a vermicule phase. I have never seen flagella enter crescent spheres; but such things are easily overlooked if not specially sought for.

(No. 35.) *22nd March*, 1898.—I was delighted to gather from your last that you are at last fairly on your life's work,[2] and I wish you all success. Very likely you will find it a hard task; but remember the experience you have already had. When things seem hopeless and you are ready to throw it up, the light may be just at hand. You are sure to throw a beam or two on many dark places even if the theory is not proved. But I think it will be. The rest is plain sailing, and may well be left to other men. . . .

Don't forget MacCallum's discovery of the flagellum impregnation of the halteridium spheres. He has a long paper in the *American Journal of Experimental Pathology* which is merely an amplification of what appeared in the *Lancet*. . . .

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[1] Flagellating body = ripe male gametocyte.

[2] Ross had now been placed on special duty.

Don't forget to work on avian blood plasmodia. Mosquitoes bite birds regularly, and they are easily experimented with, and might give you material when the human article is difficult to find.

(No. 36.) *9th April*, 1898.—Your letter of 21st March just to hand. I need not tell you how delighted I was to hear of your success.[1] Dawn seems to be near. It carries the promise of a brilliant day. Depend upon it, all your work will carry a rich reward to yourself and to mankind. . . .

It is somewhat dangerous to count chickens before the eggs are hatched, but I think that after your position is sure, you should hand the matter over to assistants. I think you could demand this from the Government. I think I could work this for you through the Royal Society. Lister, I know, would back you. My idea is that the mosquito fauna of India and of course the other malarial countries should be systematically studied by entomologists; the species described and classified. That each species should be studied in its relation to human blood parasites, especially malaria of man and animals, and also and at the same time to filaria. . . . I only hope the finishing touches will come soon so that you should have the glory and England the credit of this which will be an epoch-making discovery. . . .

The Americans will be working hard, I have no doubt, this summer, and so too will the Italians, but I think you are well ahead of them all.

I have just received reprints of MacCallum's papers and also of Opie's. I send them by this mail; you already know the principal facts.

By the way, this *entre nous*, I would not give myself away too much to ——. Send him the thing complete, but not in bits, as he might use your bricks to build his own house. I don't want to be ungenerous, but I want you to get the full credit of work which is yours and not his.

I fancy you have work enough in front of you without adding kala-azar to the burden. But were I going to do anything with the latter, I would select a dozen or so well-marked cases of undoubted disease, stick to them from first to last; examine their bloods every

[1] In infecting mosquitoes with *Proteosoma* by feeding them on infected larks.

day, and their organs when they died, and work out the clinical side of the disease thoroughly along with the pathology. . . .

I have a copy of "Amœbic Dysentery" somewhere. . . . If I come across it I shall send it to you, but don't worry over *Amœba coli* until you have done for your present problem. *A. coli* is a very small potato beside the plasmodium.

By the way, if you get conclusive proofs, I think you would do well to come home with them. You could get them brought out properly, look after your interests, and also recruit your health, and rest.

(No. 37.) 21*st April*, 1898.—I was delighted with your letter of 30th March and to learn of the material progress you are making. The mosquitoes and preparations have not yet arrived, but I suppose will turn up in due course, when I shall do my best to have what is illustrative drawn or photographed, or both. I shall have the insects sectioned and make a good search for the pigmented cells in the walls of the stomach. . . .

I write now specially to ask if you are teasing up your insects in salt solution. It struck me that as you complain of the bursting of the pigmented cells you might be using plain water merely. I had a lesson in this when mosquito-filaria hunting. The filariæ in the earlier stages in the mosquito all burst and disappeared if I used water in my dissections. . . . A curious thing about the filaria was that as the worm approached the end of its stay in the mosquito it could live and not burst in pure water. Perhaps something of the kind may be the case with *Proteosoma* towards the end of its stay in mosquito too. If this should prove to be the case, it is a fact that would indicate that the next stage of the protozoon is passed in water. Keep this in front of you.

If you send any mosquitoes with *Proteosoma* just after the feeding of the insect and when presumably the parasite is flagellating, and subsequently in the vermicule form, I might by luck stumble on a specimen with the vermicule entering the stomach-wall. If so—Eureka.

(No. 38.) 29*th April*, 1898.—The slides and three bottles of mosquitoes arrived three days ago. One of the slides is fatally smashed and several others are broken, but can be used. The mosquitoes are in good condition. I have been very busy of late and have not had time to go through all the slides critically,

but I have already seen enough to be quite sure you are on the track and that the theory is safe. Some of the coccidia [1] show up with marvellous definition, the pigment, exaggerated I suppose by the formaline, being black and sharp. I will show several of the preparations to one or two friends before having them drawn and photographed. I think I will first take them to Baker's mounter to have them ringed so as to secure against evaporation, for some of the preparations have dried and as at present are useless. . . .

I shall wait a mail or two to hear of further progress. Then I propose to write a short note for the *B.M.J.* to accompany drawings and photos. I am a little diffident about this, as you say the Government has to get your report before you publish.[2] . . . MacCallum has supplied the fertilized stomach-sphere [3] and the travelling vermicule on its way to the stomach-wall, as we of course can now interpret it to be—you have done the rest, but I would like you to find out the travelling vermicule stage for the crescent plasmodium, for proteosoma and quartan and tertian. I have an idea that I might find it in sectioned mosquitoes plunged in glycerine about half an hour after feeding. But don't worry yourself too much over details which other men can collect. If you wish to get these, apply for some young fellow to help you and set him to this work. Meanwhile push on with the advancing parasite, and try to get him the length of infectiveness. . . . You have to thank your good angel for showing you those pigmented cells in the crescent mosquitoes. And you have to thank the plasmodium that it is fool enough to carry its pigment-dung along with it into the mosquito's tissues, otherwise I suppose you would never have spotted him. A pity the pigment begins to disappear at the advanced stages, as I gather from your letters that it does. If there is no pigment in the second generation of parasites in the mosquito you will, I fear, have a difficulty in finding the beasts.

Experiments on infection you could do with birds very easily. Get nestlings, and keep them in an upper storey and away from telluric influences.

I shall be guided by your next letters about publishing. In case I have decided not to publish let me *know what you wish in this*

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[1] I.e. oöcysts.

[2] *See* footnote to Letter No. 42 (p. 172).

[3] I.e. Zygote.

matter by return mail. You must have the thing out before the warm weather comes on in America and Italy. Your pigmented cells will have put them all on the scent, and they are sure now to be in full cry. I would like their publications to come after yours as confirmations.

(No. 39.) *6th June*, 1898.—I went to see Lord Lister yesterday. He promised to go over your work and give his opinion upon it; to say whether he thought it of sufficient public importance to go with to government and tell them they must go on with it. A week ago I saw Chamberlain, who wants investigations made on the African fevers, and who is willing to make a grant for this purpose. I told Chamberlain that important investigations were in progress in India which were about to be made public and asked him to wait for a week or two, to get Lord Lister's expression of opinion on their value, before going to the Treasury for the money.

Now, therefore, send me, by return if possible, either a copy of your report to government or a précis and, if you have any very telling slides and drawings, these also, to submit to Lister. When he gives me a writing to say that it is all right, as there can be no doubt he will, I shall take this to Chamberlain and say I want money. He will probably say, Here you are. I shall then go to the India Office through Cleghorn or some one else and tell them what Chamberlain is doing. They will probably go one better. I will suggest a commission with you at the head of it, and I will get Lister to back me up. I think this is the game, but it requires to be played quickly and before the edge is off the novelty. . . . You will see that —— will have the thing all over the place soon.

The *B.M.J.* has not published my notice yet. I suppose because I sent with it several drawings of your cells.

I showed the cells to —— on Sunday. He is always full of cold water and objections. He says you should sit down and work quietly for years and years until there is not a vestige of objection possible before you publish anything. I wonder how you are to live meanwhile. It has not been his policy, at all events. No, we have to lever up the Government and the public and then you will get the opportunities. So send me at once, if you can, a statement for Lister, drawings and more slides if you can spare them. Lister was very sympathetic and encouraging.

(No. 40.) *June 24th*, 1898.—I have been and am still laid up with a severe attack of general gout, but must try and give you some idea of what has been done and what I propose to do.

In the first place, be assured that your future is safe. You have done a great work and your deed will not be forgotten. You will see from the last *British Medical Journal* that I have secured your priority in discovery, and have nipped what may have been an attempt at stealing. I must have misread one of your letters, for I certainly thought you told me you had found pigmented cells in a halteridium feeding experiment. . . . You will note that I took care to get Laveran's, Metchnikoff's, and Nuttall's confimation, deeming that the foreign man has more weight with our people than has the native whom we have always with us.

On Tuesday I got Lister to come to my house, and had some six or seven microscopes with your preparations for him to study. I was very ill at the time but struggled with clothes and told him all about the matter. He expressed himself convinced. I asked him if he would back me up in my representation to the Government. He said he would, and was everything you could desire in his recognition of the value of your discovery. Among other reasons, I asked Lister to a special demonstration because I want him to help me in the representation I propose to make as soon as I can get to the Colonial Office and India Office. I shall see or write to Chamberlain, who has already expressed a desire to do something in Malaria, and represent to him that now is his opportunity. I shall briefly explain the nature and possibilities of your discovery and suggest that the Colonial Office approach the India Office suggesting co-operation. For a reliable and the highest possible opinion on the value of this discovery, I shall request him to communicate with Lord Lister, P.R.S. (There is no fear about Lister's answer; I have sent him your report.) I shall suggest that the Colonial Government either make a grant for the continuation of the investigation or send over two men to be under and assist you, and so qualify for working out the problem in Africa and other colonies. I shall say that time, a free hand, and adequate finances are necessary. He will write to the India Office, and the thing should be as good as done. A permanent commission appointed with yourself at the head.

I would have written more fully but am horribly weak and unfit to sit at my desk—I had to get my daughter to typewrite for me.

(No. 41.) *1st July*, 1898.—I have got to the end of nigh three weeks' solid misery and pain. I am better at last, but as weak as a kitten and tired to death. Meanwhile the world has wagged on, and there has been a little progress in the mosquito-malaria business. I have got the two letters, copies of which I enclose, from Lister. These I have sent to Surgeon-General Harvey[1] with the expression of the hope that you will get a free hand, and asking him if it would be possible to get two men attached to you from the Colonial Medical Service. It will do no harm to ask; on the contrary it will show him that your work is appreciated at home. I sent him Lister's letters with the same idea. To-morrow I hope to be strong enough to go to the Colonial Office and set them on to the India Office. . . .

I am looking forward to your next work with very great interest. You are like Captain Cook now, sailing into unknown seas which may reveal wonders inconceived. "We were the first that ever burst into that silent sea" says old Coleridge; original work of the kind you are on gives the feeling that the old navigators must have had when they plunged into the unknown.

(No. 42.) *July 8th*, 1898.—I think things are going on well on this side. Mr. Chamberlain has written to Lord Lister to the effect that he wants something done in the malaria line, and saying that the Colonial Government will bear half the expense. Lister called on me yesterday and I posted him again on the mosquito business, so that he might be ready for the Committee meeting of the Royal Society yesterday afternoon which was to discuss Chamberlain's letter. I suggested two medical men, and an entomologist to go to India to help and learn from you, who might afterwards be sent to Africa or elsewhere to take up the inquiry. I think it will be all right now. At all events the Indian Government will see that whatever they think about the matter it is considered one of the first importance here.

I am having a précis of your report made, and shall read it or have it read at the B.M.A. meeting at the end of the month, and will have some six or eight typical slides on show. I would have had photos, etc., and lantern slides, but I have been too ill to accomplish this and drag myself through the daily routine as well.

[1] Director-General of the Indian Medical Department in succession to Cleghorn.

I am anxiously awaiting your next letters, for I would like to be able to say at the B.M.A. meeting [1] that the early stage and the sporulating stage of proteosoma in mosquito had been found.

When found, what next? I suppose, following the spore into other mosquitoes and into the bird. This will be delicate and difficult, but to you not impossible. This worked out, you will be ready to tackle human malaria.

I hope you approved of my notice in the *B.M.J.* I am sorry about the mistake in Halteridium, but in reading your letters I certainly had the impression conveyed to my mind that you had seen it in mosquito. . . . If the Royal Society Commission goes out to you, I shall take care that they have a complete protozoal library.

(No. 43.) *July 15th*, 1898.—I cannot make out what has become of my letters to you. Lately I have written almost weekly. Besides Opie and MacCallum's papers, I sent you a copy of my little book the day it was issued. Have you not received it? I have written regularly to keep you posted in what I have been doing to help you forward in your good work, and to let you feel that you are not being overlooked.

The last is that a committee of the Royal Society has your work under consideration. I have lent them the copy of your work, and have offered to demonstrate your specimens. The last lot arrived yesterday. I have not opened it yet. Before this I had two other lots—one set of mosquitoes fed on three degrees of infection—the other an earlier consignment of miscellaneous.

I have written the Under-Secretary of State for India for permission to bring forward your work at the British Medical Association in Edinburgh.

I am ill again and in bed with gout.—Edith M. Manson (for Dr. Manson).

Your last news of the microvermiculi is hopeful.

(No. 44.) *Edinburgh*, *28th July*, 1898.—Just a line to say I duly received both your telegrams and letters describing the germinal rods,[2] their incorporation by the salivary glands, and the successful

[1] Ross sent a copy of his Report to Manson on 13 June requesting him to make full use of it at the B.M.A. meeting, to ensure priority.

[2] I.e. sporozoites.

inoculation experiments suggested by this remarkable discovery. The news came just in time for the B.M.A. meeting, and as I was requested to give a special demonstration in malaria to a combined meeting of sections, I chose your work for my theme, and I hope did you justice to a very large audience. I had your slides under microscope and your drawings and diagrams. The fat is thoroughly in the fire now, and you may expect soon to hear more of yourself than your modesty may care for. Lister is taking a very great interest in the progress of the investigations, the India Office in various ways has also been made conscious of the high esteem in which the investigations are held—so that I think there is no need for you to be anxious about the future.

I am still ill with gout, hardly able to hobble about, but I was determined if at all feasible to come to this meeting and properly present your work, the importance of which it is difficult to overestimate. Osler was there; very much interested. I gave him one of your reports.

(No. 45.) *5th August*, 1898.—Since my return from Edinburgh I have been most of the time in bed. I am much better now, but frightfully weak and unable to tackle anything but necessary work. The R.S. proposals are not good—at all events to my mind. . . . I shall have another try for a full Commission on the original line I suggested. I hope I may succeed. Your discovery is either the most important of the "*fin du siècle*" or it is nothing. I shall do all I can for it on the first assumption, and try to make England work it.

Look in the egg of the mosquito for germinal rods. Analogy suggests such a way of indefinite propagation in mosquito.

(No. 46.) *9th August*, 1898.—The Royal Society wished to send two biologists to Africa to study malaria. They said medical men knew nothing of biology. I think I have stopped this, and have managed that one of the men at least shall be medical. I shall nominate Daniels [1] of Demerara, who has done some excellent

[1] Dr. C. W. Daniels who later became, next to Manson, the most influential teacher in guiding the destinies of the London School of Tropical Medicine. His solid contributions to science were many. Served as Medical Superintendent of the School from 1901–03 and as Director from 1905–10.

work in many directions. I think he will be first sent to you to learn your methods and help you. The research scholar in Bombay is to be sent to report on your work. Make him a convert quickly. Everything hangs on it. You have the ball at your foot if his report is favourable, as it must be.

I have just had a letter from Harvey. He is your strong supporter, and I am sure will do all he can to help you on. . . . If you get a good report from the Royal Society I fancy you will be able to make your own terms. I would advise you to get a skilled draughtsman to do your drawing in your next or final report on proteosoma. I suppose you can get such a man in India. . . . I told Lister that very few poets could illustrate their own poems. He seems to see it. They forget that Calcutta is not London. Moreover, I thought your illustrations excellent. Still we have to advance on orthodox lines, it seems. You won't mind the hint.

(No. 47.) *2nd Sept.*, 1898.—Yours announcing your failure to get the black spores to germinate to hand. *Nil desperandum.* Might not they be intended for deposit in water or soil, to be rained on, sunned on and nursed in this way for a time and then to acquire germinative properties if taken up by the proper host. . . .

It is almost fixed that the Colonial Office sends Daniels of Demerara (a good man) out to you for two or three months' instruction in preparation for joining two other commissions the Royal Society are to send to Central Africa with a special eye to studying blackwater fever and the malaria problem there. I had a great struggle to get this, as the Royal Society want to do things their own way, which, I am sure, is the wrong way. However, Daniels will work heartily with you and at all events confirm your discoveries. . . . I wish the Royal Society had followed my advice more completely, but everything, every new idea takes time to sink into clever men's brains.

You must get ready to come home when your six months are up, and be sure to come provided with a large stock of preparations. You must din the thing into the public ear over and over again before a gaining move can be started. I mean to try again, however, when opportunity offers. . . .

Koch has gone to Rome to study malaria. Pity he didn't go there before. I sent him one of your reports which the India Office sent me.

(No. 48.) *6th October*, 1898.—I was down at the Colonial Office yesterday and was told that Daniels leaves Demerara this day week. . . . You may look for him about the beginning of December, therefore. That will give him two good months with you, which, so far as he is concerned, should be sufficient to get thoroughly indoctrinated with your methods and views. . . . Above all, it will enable him to pronounce on your work and to confirm it. I believe we will get confirmation from Italy and America before that time, but it is quite on the cards that the Italians, Americans and Koch may fail to find suitable species of mosquito hosts ; in which case independent testimony from Daniels will be of great use in securing a favourable hearing for you when you come home in February. Be sure to bring with you as much material as you can lay hands on. I can't get the coccidia [1] in the sectioned mosquitoes. Some other method of preparing them must be found—some other method in which there is no shrinking of the tissues. . . . You should experiment a bit in this line, and also in trying to make preparations with ordinary balsam, as they keep so much better. A great many of your slides have perished utterly, some are deteriorating rapidly. A good many are still in good preservation, but I fear for their future. I am sending one to Rome in a day or two. I hear that Bignami and Marchiafava are going into the country to work mosquitoes, and I also hear that the American pack is in full cry after mosquito. We should have news soon. No letter from you last mail. I hope your health is all right.

(No. 49.) *30th October*, 1898.—I was greatly relieved by your letter of the 9th inst. I had not heard from you for so many mails that I began to conjure up all sorts of horrors—amongst others that you had broken down and were unable to write, or that the mosquito theory had burst up. I assure you it was a relief to be told by my wife when I came home last night that there was a letter from Dr. Ross in the hall. . . . Daniels arrived two days ago, and will leave for you on the 18th by P. and O. steamer. He is a first-class man, keen as mustard, a thorough worker, and with a broad mind and a large experience. I am sure he will prove of great assistance to you as well as to the Commission he is to join later on. After you have put him through Proteosoma you should

[1] I.e. oöcysts.

set him to work up the grey mosquito and the crescent; together you must positively settle this before you leave. I hear Koch has failed with the mosquito in Italy, so you have time yet to grab the discovery for England. I also hear that MacCallum is on the track, and working hard. . . .

I had a long talk with Cleghorn the other day about you. He said there ought to be no difficulty in getting you permanently attached as scientific investigator, and that the way to do it was through the Secretary of State. When you come home we should be able to work this easily, I hope. . . .

Be sure not to delay, unless on very urgent grounds, your departure from India. You want a rest from work and in a decent climate, and I am sure you would pick up a lot of new ideas and fresh information which would be of great service to you in your future work in India. I would retire, however, from the Service were I in your place. You are bound to be pushed forward now, and as soon as the coping-stone is put to the malaria pile, or as soon as other men have confirmed your work, which is only a matter of a few months at furthest.

I look forward with much interest to your next letters with additional information about kala-azar.

(No. 50.) *8th November*, 1898.—You are now on the mounting wave which I trust will surely lead on to fortune. Bignami should have experimented on your lines, though. He ought to have fed his mosquitoes on crescent blood, waited a week for the germinal bodies to form, and then set them to bite. I have written Charles [1] to tell him this, and I hope he will not be too late this season to try. I hope, however, you have anticipated him and that you can give chapter and verse, that is mosquito and experiment, for tertian, quartan, and crescent malaria by this time.

I am not quite so sure about this kala-azar business. I cannot disabuse my mind of the idea that possibly it is a double infection; a malaria infection with a something superadded. Why should it spread in the slow way it has done if there is not something specific about it? Your explanation may be the correct one, but it is curious that it should be so localized, should infect particular houses and villages, and should cause so fatal a form of liver and

[1] Dr. Edmonton Charles, a retired officer of the I.M.S., settled in Italy.

spleen hypertrophy. If I were you I would be careful to safeguard myself in emitting the pure malarial hypothesis. I hope you will bring home livers and spleens with you properly prepared for microscopical examination. Try for a bacterium in these organs.

I sent Bignami and Charles one of your preparations each. I grudge to part with them, but I think it is in your interest that the Romans should have ocular proof that you have been before them. . . . We will take care that your interests are looked after here.

Daniels is getting ready to leave. . . . Make him thoroughly acquainted with the proper species of mosquito, and see that he takes preserved specimens away with him. It won't do for him to go to mid-Africa unprovided with anything that might further the work there. Do you also furnish yourself with ample material for illustrating your work when you stand before the British public next March.

I hope you are feeling better and that you are able to place the top stone on your magnificent building.

(No. 51.) *16th November*, 1898.—This will be handed to you by Dr. Daniels. . . . You will find him an excellent fellow socially and a first-class man at his work. He is a good practical pathologist and clinician and, in addition, he looks upon disease from the standpoint of the naturalist. This, I take it, is the right way. His capacity for original work has been enhanced by the valuable circumstance that he has had to trust to himself for many years. There is no better training for a capable man. You will, I have no doubt, give Daniels a lot; I am sure, however, you will get from him a lot in return.

I am looking forward with interest and expectation to your next letters. The mosquito theory is now fairly on the ascendant; thanks entirely to you. You will be lionized when you get home.

(No. 52.) *7th December*, 1898.—I know that Charles has posted you in the progress of events in Rome. You will see from the *Lancet* the position Bignami is taking, and how there is an attempt to belittle you while exalting himself. All this is very amusing and contemptible. Hold your own course and don't be influenced by it. You have plenty of friends here who know how to uphold you and your rights. Your work is the starting-point of all

this mosquito business; this cannot be undone. Meanwhile, do your best to prove by set experiment on man that what holds good for proteosoma holds good for the human malaria parasites. Your being sent kala-azaring was a misfortune and has lost you valuable time. Now you have Daniels at your side, you will have fresh incentive to push on and most valuable assistance. He is the right sort, I feel sure.

(No. 53.) 10*th January*, 1899.—I am very sorry to hear you are so seedy. You must clear out at once if your health is giving way. You are much too precious to lose now, and there is not much to be gained by your remaining in India longer. You have forged the key and others must take the trouble to open the door. The Italians are hard at it, as you will learn from Charles's letters, and we will take care here that your position in this matter is not ignored. Daniels should by this time be fairly indoctrinated with your views and methods, and he is of a kind that wants only to be put in the way of work to do it and do it well. That kala-azar business was most unfortunate. Had you been left alone you would have anticipated all that the Italians are doing. *Practically you had already done it, and you can justly claim to have been the first to find the malaria parasite in the mosquito as well as to point out the modus operandi of that insect in diffusing malaria.*[1] All the detail will come in time, in a very short time I believe. Therefore if you are not quite well, start for home at once, and chuck for the time being the kala-azar report. You can finish it when you get home and a rest.

Two mails ago I wrote to Harvey telling him that I hoped he would get the Indian Government to make some recognition of the splendid work you had done. I pointed out that its enormous importance was already recognized in Europe, and that honours were in store for you here. I told him it would be a graceful act for the Indian Government to anticipate these. . . .

I am glad you approve of Daniels. He ought to be of great assistance to you. I think he will do good work in Africa, for he has the knowledge and the spirit and the ability.

(No. 54.) 18*th January*, 1899.—The Romans are still active. I wrote last week to Bignami telling him we were careful of your

[1] Our italics.

reputation and hoping that he and his compatriots would be equally so. They are all on the grab, it would seem. I had a letter from Charles to-day saying that there are now some twenty or thereabout claimants for the mosquito theory. I daresay in another month there will be fifty. It is very amusing as well as ridiculous.

I have written Daniels thanking him for his letter and suggesting he should try the artificial marsh. He will probably show you what I have written. I have also asked him to get me halteridium and proteosoma slides. I can't get them here. Don't you bother about truck of this sort, but keep your energies for the weightier matters. I hope before you leave that the human malaria will be as well worked out as the avian, and that the black spore business will be solved.

But don't let your wish for completeness keep you a day longer in India than your health will easily stand. Don't risk anything. Your work is practically complete, and you should come home for a time.

They are getting up a tropical school in Liverpool, and came to me about a lecturer. Amongst others I suggested your name, and Boyce has jumped at the idea. I don't suppose you would care for it. But I encouraged him to try for you in the hope that the Indian Government would make a better bid. I think Boyce will telegraph an offer. Go with this telegram and letter to the I.G. and ask them if you should accept. This will bring them to make you a good offer. Pardon my Machiavellianism; but it is in your interest and in the interest of Tropical Medicine I have intrigued. I would like to have you at home, but I would prefer to have you in India with a proper laboratory, a good position, and a free hand. There is so much work to be done there and very few who can do it.

These are the last letters that Manson wrote to Ross in India. We think that our readers will fully endorse Ross's opinion that these letters are "a noble series such as few men have received."[1]

[1] Ross's "Memoirs," p. 354.

SURGEON-GENERAL JAMES CLEGHORN TO MANSON

Calcutta, 27 *Jany.*, '98.

MY DEAR MANSON,—The Govt. of India have now sanctioned my original proposal regarding Ross. He is to be placed on special duty under me on Rs. 1000 per mensem. We have now to get the sanction of the Madras Govmt to his transfer. There may be some delay about this at present. I will wire him.—I remain, Yours sincerely, J. CLEGHORN.

LORD LISTER TO MANSON

12 *Park Crescent, Portland Place*, *29th June*, 1898.

DEAR DR. MANSON,—I return the copy of Dr. Ross' report, with best thanks for the opportunity of reading it.

Taken in conjunction with the preparations which you kindly shewed me, it seems to me to be of remarkable interest and value; and I regard it as most desirable that his experience, and I may add his manipulative skill, should be turned to further account by opportunity being given him of continuing his researches on this subject.—Very sincerely yours, LISTER.

SURGEON-GENERAL ROBERT HARVEY TO MANSON

S.S. Purnea, 16. 1. 99.

MY DEAR MANSON,—I got your letter just as I was starting for Burma on an inspection tour. Before leaving I had passed Ross's leave, and I hope to be able to re-employ him on Special work when he comes back. . . . I note what you say about recognition of his services. I had already thought of sending in his name. . . . Yours sincerely, R. HARVEY.

A few words must be said in conclusion about Manson's rather unfortunately designated "Experimental Proof of the Mosquito-Malaria Theory," which was carried out partly in London and partly in Italy, in 1900. Manson's design was to give a practical demonstration of the theory such as the ordinary man, who takes no interest in science apart from its utilitarian applications, could understand; but by the unhappy omission of an initial indefinite article from the title, *an* experimental

proof by Manson in 1900 has sometimes been confused in significance with *the* experimental proofs in 1897–8 by Ross.

Manson knew that the ordinary man, even if "well-informed," is not moved by the logic of the science laboratory but requires a sign that can be avouched, if not by his own eyes, at any rate by his newspapers. He therefore had some *Anopheles* mosquitoes that had been infected with malaria parasites (benign tertian) in Rome brought alive to London, where they were set to bite two healthy Londoners—one of them Manson's son; with the result that, after the due period of incubation, both victims had fever, both had benign tertian parasites in their blood, and both afterwards suffered from characteristic relapses of fever. Here indeed was a sign for a perverse and foolish generation.

Conversely, he organized an expedition to prove to the plain man that people who are well protected from those insects' bites can live with *Anopheles* mosquitoes in a malarious country without getting malarial fever. For this expedition, which was conducted by Dr. G. C. Low, Dr. L. W. Sambon, and Signor Terzi, a wooden hut, constructed in England, was erected near Ostia, at the mouth of the Tiber, in the Roman Campagna, at a spot known to be intensely malarious. The doors and windows of the hut were made mosquito-proof by wire-gauze and the occupants' beds by mosquito-nets also. Drs. Low and Sambon and Signor Terzi, with their Italian servant, went into residence early in the month of July and remained, throughout the malaria season, until 19 October. They went abroad freely during the day, but were careful to keep inside their mosquito-proof hut from an hour before sunset until an hour after sunrise—the period when *Anopheles* mosquitoes

are active. In consequence they remained in perfect health throughout the whole term of their stay, in startling contrast to the local native peasantry and the unprotected doctors and attendants of the Red Cross Malaria Service at the same time. Here, again, was an omen for a generation unresponsive to pure science.

Manson's exalted place in the history of the discovery of the transmission of malaria is not, as some appear to have thought, based on these practical demonstrations of an already-verified scientific proposition. It is based primarily on his astonishing preconception of the significance of the " exflagellation " of the Plasmodium as an adaptation to its extra-corporeal existence; and secondarily on his legitimate inference from his own earlier discovery of the manner of transmission of another vascular parasite of man (*Filaria bancrofti*), that this extra-corporeal existence of the Plasmodium was staged in a mosquito. Before Manson came upon the scene the solution of the problem of malaria transmission had appeared hopeless even to Ross.[1]

What Manson intended in his " Experimental Proof of the Mosquito-Malaria Theory " is very plainly manifest in the following extract from the introduction to his paper on the subject:[2]

" Although the theory that the malaria parasite is transmitted from man to man by a particular species of mosquito is now accepted by all biologists and medical men who had given adequate attention to the subject, it cannot be said that the general public (including those Europeans who in malarious countries might benefit by the practical application of this theory) unreservedly believe in, much less practically apply it. Endless objections the outcome of imperfect acquaintance with the subject and, perhaps, of a

[1] *Nature*, vol. lxi., 1900, p. 522, col. 2, l. 60.
[2] *British Medical Journal*, 1900, ii., 949–51.

disinclination to admit that a pathological puzzle of so many centuries' standing could receive so simple an explanation, have been raised by the amateur biologist and sanitarian, so much so that it seemed not improbable that a great principle, pregnant with important issues, might remain barren and unutilized. Impressed with this fear, and being anxious to see some fruit from a theory which I knew to be true, and for which I was in a measure responsible, I cast about for means by which the conversion and co-operation of the public might be secured. I felt that unless the public believed in the efficiency of the sanitary measures so definitely indicated by the mosquito-malaria theory, and understood the principles upon which these measures should be founded, they would not adopt them, nor, what is so necessary to the success of all measures, co-operate heartily in carrying them out. As the histological, biological, and experimental evidence which had proved satisfactory to men of science was not understood by the public, it seemed to me that some simple demonstration, such as would be unanswerable and at the same time readily comprehended by laymen, was required. Grassi, in conjunction with Bignami, had succeeded in conveying malaria by mosquito bite. Although these experimenters took every care to exclude fallacy, the fact that the experiments were made in Rome, itself of fever repute and in the middle of a highly malarial district, had an undoubted influence in preventing due appreciation by the public of the conclusive nature of their work. Furthermore, things occurring at a distance and in a strange land do not appeal so strongly as do things happening in our midst. It occurred to me, therefore, that if I repeated Grassi and Bignami's experiments in a more dramatic and crucial manner, that if I fed laboratory-reared mosquitoes on a malarial patient in a distant country and subsequently carried the mosquitoes to the centre of London, and there set them to bite some healthy individual free from any suspicion of being malarial, and if this individual within a short period of being bitten developed malarial fever and showed in his blood the characteristic parasite, the conclusion that malaria is conveyed by the mosquito would be evident to every understanding, and could not possibly be evaded.

" It also occurred to me that if a certain number of Europeans who had never suffered from malaria kept in good health and free from malaria during an entire malarial season in an intensely

malarial locality, where all inhabitants and visitors suffered from malaria, and if they kept well without the use of quinine or other medicinal prophylactic, simply by avoiding mosquito bite, the above conclusion would be accentuated; and, also, that if this immunity were attained by inexpensive means—means which did not interfere seriously with comfort, pleasure, or business—the mosquito-malaria theory would not only be proved to the satisfaction of the public, but the public would be willing to accept the sanitary measures which the theory and experiments indicated.

"After having obtained promises of support from the Colonial Office and from the London School of Tropical Medicine, and having secured volunteers for the experiments, still further to accentuate my object and to arrest the attention of those principally interested, I publicly announced in a popular lecture at the Colonial Institute that the above experiments were about to be undertaken, and with the same object in view I ventured to forecast their issue."

CHAPTER XI

Further Contributions to Science (1891–1910)

One of our chapters has been devoted to Manson's work on filariasis at Amoy, and two others to his mosquito-malaria theory, while in Chapter VIII we have related the chief incidents of his life in London from 1890 to 1907. We must now record other contributions to science which he made during and after those fruitful years. One of his earliest preoccupations in London, as we have already noted, was with filariasis, a subject in which there was still much that remained obscure. By dint of enterprise he managed to obtain blood specimens from workers in the field and, occasionally, an opportunity presented itself of examining natives from the West Coast of Africa, and other parts of the tropics, who happened to stray to England. His efforts were soon rewarded with success. Early in 1891 he was able to report the discovery of two new species of filaria, or blood worms, in two natives of the Congo and in three negro boys from Old Calabar whom he found in an East End Mission.

The larger of these two worms he named provisionally *Filaria sanguinis hominis major*.[1] In many respects it resembled the embryo of *Filaria bancrofti*, with which he was familiar. It was provided with a sheath, but,

[1] Subsequently known as *Filaria diurna*, and now recognized to be the embryonic stage of *Loa loa*.

unlike the other species, was found to circulate in the blood solely during the daytime and to disappear entirely from view at night. Further, the " periodicity " of the parasite could not be reversed by altering the habits of the host, as in the case of the embryo of *Filaria bancrofti*. The mechanism of " periodicity " could not be explained by changes in the circulation during the night or during the day, for he had now demonstrated that the phenomenon was exhibited by two distinct species of filaria which, though practically identical as regards size, shape, anatomical features and movement, exhibited exactly opposite periodicities. Here was a pretty problem : it was one of which the solution might well appear to be insoluble. The diurnal habit of the parasite indicated to his mind a corresponding diurnal peculiarity in its intermediary host, just as the nocturnal habits of the *Filaria sanguinis hominis nocturnalis* (or the embryo of *F. bancrofti*) pointed to the mosquito. As the diurnal species, owing also to its anatomical structure, was unable of itself to escape from the body of its host, the aid of some blood-sucker was necessary, and, conforming to the habits of the filaria, this blood-sucker had to be of diurnal feeding habits. On questioning one of the most intelligent of the natives about the day-biting blood-sucking flies of Old Calabar, Manson was interested to hear of two flies, a red and a black, called respectively in the Old Calabar tongue *uvo* and *ukpom*, whose habits seemed to qualify them for the rôle of intermediary host to this filaria. The flies, known as mangrove flies (*Chrysops*), were said to be common around the plantations, creeks and rivers of the Gold Coast. They attacked the skin with great pertinacity, gorging themselves with blood to such an extent that they were unable to fly. In a letter to Dr. Grattan

Guinness in January, 1891, Manson fully explained all these points, giving, indeed, a remarkable forecast of the life-history of *Filaria loa*, proved by Professor Leiper to be correct twenty-one years later. Not only did the main hypothesis turn out to be correct, but the actual species of *Chrysops* is the one now generally recognized as carrier of the infection. A specimen of *Chrysops dimidiata* caught in the act of biting a native on 16 April, 1892, may still be seen in the British Museum, to which it was presented by Manson in April, 1893.

In his endeavours to work out the truth of his hypothesis he had arranged with various missionary workers to send him mangrove flies killed at intervals after feeding on negro blood and preserved in Müller's fluid. There is no evidence, however, that these promises were ever fulfilled, so that Manson was unable to complete this work.

The second species of filaria was a much smaller worm and, like the corresponding parasite of the dog (*F. immitis*), which he had also studied in China, was sheathless. It was the first species discovered in man which did not observe the habit of "periodicity," but continued to circulate in the blood both day and night. Provisionally known as *Filaria sanguinis hominis minor*, its cognomen was soon altered by him to *Filaria perstans*. Although not known to possess any pathogenic properties, its exact significance could not be appreciated till the discovery of the adult stage had been made some six years later by Dr. Daniels in British Guiana; this was recognized and described by Manson himself.

In searching for the probable cause of sleeping sickness in Central Africa the discovery of *F. perstans* led him on a false scent which he continued to follow for a number of years. He was impressed by the circum-

stantial evidence of the association of *F. perstans* with sleeping sickness, which he had found in all the cases of that disease which he had seen in England. He did not specify the exact way in which such a parasite could give rise to sleeping sickness, but he imagined it might stray from the safe position it occupied and in its wanderings might find its way to the blood-vessels or lymphatics of the brain and thus give rise to fatal lethargy. This minute species was provided with a retractile spike at its anterior extremity, with which he thought it was enabled to bore through the walls of blood-vessels and thus find its way into the grey matter of the brain. Further than this Manson was careful not to commit himself. In 1900 he pointed out that, although there were strong grounds for connecting *Filaria perstans* with sleeping sickness, on account of the remarkable correspondence of the geographical distribution of the parasite and the disease, yet there were striking exceptions, especially in the Congo basin, which made him doubt the validity of his former views.

In response to his appeals for further material he soon received a number of blood films from Cochin China, the Congo, Cameroons and the Gold Coast. In 1893 he examined at the Crystal Palace the blood of a troop of natives from Dahomey and was rewarded by the discovery of *F. perstans* and *F. diurna* in a considerable proportion of them. It was significant that in countries lying just beyond the tropical zone blood filariæ were not to be found. In order to make this point certain he examined blood slides of a large number of Kaffirs and Hottentots which he obtained from Dr. Harley of Capetown. In 1894 we find him in correspondence with Dr. Davies of Samoa regarding the excessive

prevalence of elephantiasis in the South Sea Islands. Other consignments of slides came from Ceylon, Rangoon, Nigeria, Morocco, from New Guinea, Demerara and the West Indies, and were carefully examined. These researches were soon to be rewarded by fresh discoveries. In December, 1897, through the assistance of Drs. Ozzard [1] and Daniels, who collected blood slides from the aboriginal Carib Indians, Manson was able to describe certain new species of blood worms occurring in South America; these were two distinct forms of sheathless filariæ which he named respectively *Filaria ozzardi* and *F. demarquayi* (the latter at Blanchard's suggestion). A few months later the adult form of one of these nematodes was demonstrated by Dr. Daniels in British Guiana.

No further material facts in connexion with these parasites have since been forthcoming, and the subject remains much where he left it thirty years ago.

Manson regarded the ecdysis—the discarding of the empty sheath of the embryo, not as a normal feature in the life-history of filariæ, but as a necessary interlude in the life outside the human body. When filaria-containing blood slides were placed overnight in frosty weather near a window, he found next morning that many of the filariæ had quitted their sheaths owing to the thickening of the blood. It was evident that the mechanism was an entirely mechanical one, the viscid blood holding the sheath while the little parasite inside could effectively charge, ram and burst through it in much the same way that a snake rids itself of its slough by travelling through grass or brushwood. The significant feature of these chilling experiments was that they produced artificially what actually took place in nature.

[1] Dr. F. A. Ozzard, now resident in British Guiana.

In the filariæ artificially deprived of their sheaths he could make out with great distinctness the organs by which the parasite pierces the tissues of the mosquito. Watching a moving sheathless filaria, he observed at once that the extreme end of the head is in constant and very active movement, that it is provided with a thick stumpy tongue, or proboscis, which is covered and uncovered by a six-lipped rectractile prepuce from which a short and extremely delicate filament is shot out and retracted. He interpreted the filament as a feeler or sense organ, the tongue as a borer, and the prepuce as a dilator and holdfast.

Each one of these organs was of use to the embryo filaria during its course of development in the mosquito. The sheath itself he explained as the remains of the egg-shell of intra-uterine life. He believed that the use of this encumbrance, for such it is, is to prevent the filaria from using its formidable boring apparatus on the walls of the blood-vessels of its human host—not, however, in the interests of the human host, but for the well-being and future of the parasite itself.

In 1894 he was able to make a considerable contribution to the knowledge of the minute anatomy of the adult worm—*Filaria bancrofti*, which, though discovered eighteen years previously, had never been accurately described. He received seven more or less complete specimens, three males and four females, from Surgeon-Major Maitland, of Madras, who had excised them from a mass of distended lymphatic vessels. By this study he was able to make out the structure of the tail end of the male worm, which had hitherto not been seen in an undamaged specimen, and which is the distinctive character of this species. The accurate

drawings he made at this time have since formed the basis of the textbook figures of *Filaria bancrofti*.

Two years later an unlooked-for opportunity of studying in London the adult filaria worm, while still living and active, presented itself. In December, 1896, Manson was requested by his colleagues to see a case in Charing Cross Hospital. The man had a small tumour in his arm, which was thought to be filarial in origin. On its removal a female filaria emerged and survived for twelve hours, swimming about in a normal saline solution quite actively, like an eel. The demonstration of this specimen to a class of students at the hospital excited enthusiasm; it was probably the first time that such a spectacle had been seen in London, and it is doubtful whether it has been repeated since. In 1897, as recorded in Chapter IV, p. 48 (footnote), this same patient, who suffered from various manifestations of filarial disease, and in whose blood embryonic filariæ abounded, committed suicide at 8.30 in the morning by swallowing prussic acid. At the post-mortem valuable information on the intermittent distribution of the embryonic filariæ in the peripheral blood was obtained. Death occurred at a time when no embryo filariæ were present in the peripheral blood, so that it was an opportune moment for finding the place to which they retired during the day. By far the greatest numbers were found in the capillaries of the lungs. In the sections of this organ the filariæ lay outstretched or variously coiled in the vessels, large and small; in the heart muscle they lay along the capillaries between the fibres; in the kidneys they seemed especially to affect the Malpighian tufts; and a few were found in the capillaries of the brain. A number of adult filariæ were found in a lymphatic cyst in the retroperitoneal tissues.

Having many opportunities of examining patients from the West Coast of Africa, Manson was interested in the pathological effects attributable to infection with *Filaria loa*. In 1902 he had suggested that there was a close association between this worm and the curious evanescent tumefactions known as " Calabar swellings." It was not until eight years later that he had an opportunity to test his views. Calabar swellings were in some way brought about by the adult worm, or its embryo. That the former did not usually give rise to much irritation or to pronounced swellings he was convinced, for months might elapse, the worms all the while moving about the body, yet no swelling might appear. One of his patients was a man whose blood teemed with *loa* embryos, and who used to amuse himself by harpooning with a needle the adult worms as they wriggled their way under the skin on his chest or abdomen. He became quite an expert at this form of sport, and on three occasions sent Manson the fruits of his spear in the shape of fragments of mutilated worms which he had succeeded in impaling. Evidently this man was the host of a large number of these parasites and yet, though the worms were active as well as numerous, it was only at long intervals that a Calabar swelling appeared. The mere presence and movements of the worms, therefore, did not cause the characteristic œdema. Manson's conclusion was that the swellings were produced by the normal emptying of the contents of the uterus of the gravid female worm into the connective tissues. One evening the patient turned up with a Calabar swelling on the back of his hand which had commenced some six hours earlier. On aspirating the œdematous tissue with a hypodermic needle, Manson obtained a few drops of effused serum in which there

were large numbers of embryo filariæ, and thus his theory found confirmation.

Not the least of Manson's contributions to science was his confirmation, in 1894, of Fedchenko's[1] original observations (1869) on the development of the guinea-worm embryo in *Cyclops*. The cyclops, or water fleas, used in his experiments were procured from Wandsworth Common and from Birmingham; the embryos of the guinea-worm from a patient under his care at the Albert Dock Hospital. A supply of these active wriggling organisms was readily obtained by squeezing a little cold water within an inch or two of the guinea-worm ulcer on the patient's leg. The embryos were put into bottles, together with a number of cyclops, and on observing them twenty-four hours later he found that nearly every cyclops contained from ten to twenty guinea-worm embryos. The parasites had not entered the body cavity through the alimentary canal but had penetrated the armour of the crustacean between the joints of the ventral plates. The embryo underwent considerable changes in shape and size, moulted twice, and in one case, at least, was observed to live in the cyclops for at least seventy days after its introduction. The drawings and microphotographs made from his specimens at this period fully illustrate the metamorphosis of the larval stage of the guinea-worm. In a further contribution to the knowledge of this subject he recorded (1896)

[1] A. P. Fedchenko, "O stroenii i razmnozhenii rishtui (*Filaria medinensis*, *L.*)." Izvyest. Imp. Obshchest. Lyubitelei Estest. Antropol. i Etnoghr. Moskva (Bull. Imp. Soc. Friends of Nat. Hist. Anthrop. Ethnogr. Moscow). Tom. VIII. Chast. 1, Protokol. zasyed. Obshch. (Protocol of the sittings of the Society) ghod. 7, 1869, coll. 71–82, 1 pl.

the successful extirpation of the adult worm in the human subject by the injection of corrosive sublimate solution. This method of treatment was in his opinion greatly superior to the time-honoured process of winding out the protruding worm by attaching it to a piece of wood. The simile he employed in developing his argument was particularly apt. He pointed out that if a sailor wants to control the paying out of a cable, he takes a turn or two of the cable round a "bit." The resistance thus obtained is so great that with one hand he can control the traction of many tons weight, and, should he choose, he can hold on till the stout cable snaps. Exactly the same mechanical principle applies to the winding out of a guinea-worm, which is fortified by its hook-like tail; this corresponds to the sailor's arm, the folds and kinks of the worm among the tissues to the "bit," and the traction exercised by the surgeon to the force dragging at the cable.

In 1903 an opportunity occurred of determining the life-span of the guinea-worm. In May of that year Manson saw a patient who had a guinea-worm in his leg, and within four days he saw another in whom the parasite had presented in the axilla, and he elicited the fact that these patients had been companions on a shooting trip to Uganda about a year before. From the simultaneous appearance of the parasite in these individuals, he was able to infer that the worms usually take about a year to present themselves at the surface of the body.

Over and above the contributions to the scientific solution of the transmission of malaria from man to man which have been related in Chapters IX and X,

Manson was able to make some solid additions to the then existing knowledge of the Plasmodium.

From the very moment that he realized the import of Laveran's discovery, he had searched the blood of malaria patients, but in vain. Failure to find what to modern eyes is an obvious object was to a great extent to be ascribed to the stain employed to " show up " the most prominent features. It had not been sufficiently realized that methylene blue, the familiar colouring medium of the period, possessed little power of differentiating various microscopical structures, and therefore it was that not until 1892 was Manson able to distinguish clearly the parasite of malaria. His success was mainly due to his discovery that when basic methylene blue (2 per cent.) is mingled with borax (5 per cent.) it acquires the property of picking out the chromatin of the parasite. It was by means of this, " Manson's stain," as well as by the judicious use of carbol-fuchsin, that he was able, in 1897, to demonstrate the stages of exflagellation. His borax-methylene-blue stain was afterwards elaborated by Romanowsky and Leishman into the highly efficient dyes which now bear their names.

Though Manson's stain, as an essential feature of the armamentarium of the laboratory, has long been forgotten at home, it is still appreciated by some Continental pathologists. The writer well remembers, when searching a German laboratory at Nazareth, Palestine, in October, 1918, shortly after its evacuation by the Turkish Army, coming across a bottle labelled " Mansonische Färbung," which bore traces of having had a useful career.

Manson's originality of technique was further exemplified by the ingenious " moist chambers " which he fashioned out of damp blotting paper. No sooner was

a slide bearing a drop of fluid malaria blood inverted and pressed on to this paper than the crescent forms began to exflagellate. At intervals the slides were dried over flame, fixed with absolute alcohol and stained. Thus he was able to record the consecutive phases of the flagellating body and to obtain the permanent preparations upon which the mosquito-malaria hypothesis was based.

In 1902 Manson reported the discovery of a case of bilharziasis from the West Indies which he thought was caused by a new species of fluke. The patient was an Englishman who, while in Antigua, was seized with a chronic dysentery and in whose evacuations he found an egg differing considerably in shape and form from the terminal-spined variety with which he had been familiar in the African infection. The conjecture proved to be well founded, and it was proved by Leiper, some thirteen years later, that this distinctive egg is produced by a separate species of fluke, named by Sambon *Schistosoma mansoni*. As a result of this discovery Manson conjectured that zoologists might be able to determine some mollusc or anthropod which the West Indies and Africa had in common, and thereby indicate the long-sought intermediary host of the bilharzia parasite. Some three years later his interest in these parasitic trematodes was renewed by the discovery by Catto of a third species in man—the *Schistosoma japonicum*; and in establishing the pathology of this disease he took a prominent part.

Apart from his conjecture of the part played by *Filaria perstans* in sleeping sickness, and his description of the clinical manifestations of "negro lethargy,"[1]

[1] An earlier designation for sleeping sickness.

Manson's first serious contribution to the subject of trypansomiasis appeared in 1900 in collaboration with Dr. (afterward Sir) F. W. Mott. From observations made upon the cases of this enigmatical disease which had been brought to England he noted that it was characterized by three stages—the incipient, the declared, and the final; that in the second of them the patient takes food whenever it is brought to him, but is slow in swallowing his meals and may even fall asleep in the process of eating. He noted also the great irritability of the skin and the general glandular inflammation. The pathological degeneration of the central nervous system was well described by Mott.

Before this, in April, 1896, Manson had been in correspondence with Surgeon-Captain (now Sir) David Bruce, who was then working in Zululand on the transmission of the trypanosome in the antelopes and big game of Africa by the tse-tse fly. In his letter he makes this significant remark: "Comparative pathology is going to throw a flood of light on human pathology by such investigations as those you are engaged on. I am very glad we have not left it for a Frenchman or a German to find out what tse-tse disease is." By January, 1897, he had formulated definite views as to the transmission of the trypanosome, for he writes: "I believe the trypanosome, like malaria, has a life-history outside the human body." In 1903 Manson was one of the first to observe the parasite of sleeping sickness—*Trypanosoma gambiense*—in man, and one of the first to diagnose human trypanosomiasis in a European and to describe the initial symptoms. The steps in the history of the recognition of human trypanosomiasis as a distinct disease appear to have been as follows:

In May, 1901, Dr. Forde, a Colonial surgeon on the

Gambia, examined the blood of an Englishman who was suffering from an irregular and chronic fever. Whilst examining his blood with the microscope he found an organism which was very active and which he failed to recognize. Dr. Dutton, when it was shown to him, at once recognized the nature of the parasite and realized the importance of the discovery. He named the parasite *Trypanosoma gambiense*, and the disease trypanosomiasis. In August, 1902, Manson had the opportunity of seeing a case of the disease in Liverpool. To him the clinical picture was so striking that he wondered how it was that its special nature had not long before been recognized. Two months after this he was consulted (in October, 1902) about a case of chronic fever which had long been supposed to be malarial in origin. The patient, a lady missionary from the Upper Congo, gave a history of having been bitten in the leg by a fly on 14 August, 1901. When first seen in October, 1902, fourteen months after the onset of the disease, she was very weak, her face was puffy, her spleen enlarged, and scattered over her limbs and trunk Manson recognized the same type of erythema that he had seen in Dutton's case. Suspecting the nature of her disease he sent her to hospital, and after a week's intensive search of her blood Dr. C. W. Daniels found a solitary trypanosome, but subsequently many more were demonstrated. This lady succumbed to the infection about a year afterwards, in October, 1903.[1] With his remarkable memory for clinical cases, and possibly calling to mind an almost similar occurrence in the case of the lung fluke (*see* p. 71), Manson recalled that some years before he had seen another case with exactly similar symptoms. This

[1] It was from this case that Mott described the characteristic perivascular changes.

patient was also a missionary from the Congo, who had returned there. On writing for information about her he was told that her symptoms continued and that Dr. Broden had found the trypanosome in her blood. On 12 November, 1902, Castellani made the discovery that the trypanosome occurred in the blood of natives of Uganda as well as in the cerebro-spinal fluid of those afflicted with the disease then known as sleeping sickness. Only then was it realized that human trypanosomiasis and sleeping sickness are but different stages of the same disease. Although he never made the story public, Manson would relate how he was foiled in the discovery of the human trypanosome. In 1900 (one year before Forde), whilst examining a slide of fresh blood under the microscope, he saw a tadpole-like object flit across the field of vision. The patient, who had by this time left the house, was traced to his club, but had become so inebriated and pugnacious that attempts to obtain further specimens of his blood had to be abandoned.

In 1908 Manson published his experience of the treatment of trypanosomiasis in seventeen cases. The conclusions he drew were that trypanosomiasis in man is not necessarily a fatal disease, and that atoxyl, in large doses, has a marked effect in checking the clinical manifestations and in causing the parasites to disappear from the blood.

The discovery of ankylostomiasis in the tin-mines of Cornwall in 1902 excited some comment in medical circles, since it demonstrated that, given the necessary conditions, parasites indigenous to tropical countries were capable of diffusion in England. Prof. S. J. Haldane was asked to investigate the ventilation of

these mines, mainly on account of the considerable number of cases of anæmia which had occurred there, but in consultation with Manson it was soon discovered that the anæmia was due to infection by the ankylostome worm, an infection probably imported by miners from Africa and the Malay States who had returned to work in Cornwall. It was an interesting example of the simple solution of what originally appeared to be a complicated problem.

Manson was one of the first to recognize the importance of the parasite described in 1903 by Leishman and Donovan, and now known as the Leishman-Donovan body. In January, 1904, he published a paper with Dr. G. C. Low in which they stated that, after the discovery of the trypanosome as a factor in tropical pathology, it had occurred to them that possibly this or some similar organism might be responsible for the condition of certain patients from India who were emaciated and had enlarged spleens. In one case under their care which ended fatally they had no difficulty in demonstrating the Leishman-Donovan bodies in the bone marrow and spleen. In December, 1903, they were able to demonstrate these bodies by spleen puncture, during the life of the patient. Their suggestion that the parasite could escape from the human body viâ the intestine has not since proved to be correct.

In the following year Manson developed a thesis identifying the Leishman-Donovan organism of kala-azar with the parasite of oriental sore. The latter disease, when inoculated, was found to be protective against the former. The facts suggested to his mind that oriental sore might be in the same relation to kala-azar as vaccinia to smallpox, and that possibly the kala-azar

germ had lost its virulence during its passage through the camel, just as the smallpox germ was deprived of its virulence by passage through the cow. In 1907 he put these ideas to the test and endeavoured to inoculate a kala-azar patient, who was in an advanced stage of the fever, with material derived from an oriental sore; no visible result followed and the man left hospital to end his days at home, specific treatment having proved unavailing. Within four months, however, much to his own surprise, he regained his health. This was the first recorded case of spontaneous recovery from kala-azar.

To the knowledge of the natural history of oriental sore Manson contributed observations on the lengthy incubation period, which might be as long as five months, and insisted on the probability that a winged insect such as a mosquito might be the carrier of the infection from man to man.

Blackwater fever, the most dramatic and fatal of all tropical fevers, was one in which Manson took an abiding interest. In various publications from 1893 onwards he advanced reasons for considering it to be a disease which, though in some way connected with malaria, was not necessarily due to the same parasite. This attitude, on the necessity of further research on the distinctive etiology of blackwater fever, he maintained to the end of his active career.

On the vexed question of the part played by quinine in blackwater fever he maintained an open mind, and with Dr. G. C. Low was able to record in 1903 a case of experimental quinine hæmoglobinuria in a patient who had been the subject of blackwater fever. By a deliberately planned experiment, the patient developed

a typical attack of blackwater after taking ten grains of quinine.

Although Manson did no original work on beriberi, he continued to formulate conjectures on its true etiology and to stimulate research. In 1901 he clearly stated his views that beriberi was a pure intoxication, produced by a poison elaborated by a germ whose nidus was located outside the human body; in this respect he considered that beriberi was on all fours with alcoholism, the germ of which is the yeast plant, the nidus, solutions of sugar, the toxin, alcohol, and—to complete the parallel—the pathological effect, peripheral neuritis. In 1907–08 he was in close correspondence with Dr. A. T. Stanton, who was then engaged in dietetic researches in the Malay States with special reference to rice. In February, 1908, he wrote a letter which crystallizes his ideas:

MANSON TO DR. A. T. STANTON

I am delighted at the thorough way in which you are carrying out the rice experiments and at the prospect of something like a positive result. Don't commit yourself, however, until you are quite sure. Manifestly the question is a very intricate one, and there are to my mind not a few things difficult to reconcile with this rice hypothesis. Pay as much or more attention to the facts against as those for rice. But why should beriberi be institutional in its occurrence? I know missionary schools in China where it has recurred over and over again, just like the measles. Why did it occur in Dublin? Possibly there are several kinds of neuritis called beriberi. Peg away, clear away all clouds.

On the etiology or treatment of dysentery Manson wrote little, but he fully appreciated the value of ipecacuanha and elaborated the method of its exhibition in chronic amœbic dysentery and in liver abscess. He

was convinced, even during his years of practice in Hongkong, that there were two forms of dysentery, one of which was influenced by ipecacuanha, while the other was not. He was in the habit of employing the drug in large doses in the post-operative treatment of liver abscess, and would cite the case of a patient who had been operated upon on several occasions for recurrent abscesses of the liver with sinus formation. One day, having heard Manson lecture on the value of ipecacuanha, this man discharged himself from hospital with several sinuses still unhealed, went home and swallowed large doses of the drug, and three weeks later presented himself with his wounds all soundly healed and once more restored to health.

In 1894, with Dr. Galloway, Manson reported the discovery of amœbæ in liver pus which he believed to be the first instance recorded in England. His conclusion was that amœbic dysentery and abscess of the liver were but different manifestations of the same disease.

In January, 1897, Manson published a valuable summary of existing knowledge on bubonic plague which, while embodying no original observations, yet contained reflections the truth of which have been borne out by subsequent investigations.

In 1898 he expressed his views on the possibility of acclimatization of Europeans in tropical countries. He had formerly shared the pessimistic opinions about tropical colonization by the white races, but his ideas had undergone a revolution since the establishment of the germ theory of disease. He believed that heat and moisture were not in themselves the direct cause of any

important tropical disease; also that there was a weak and unprotected point in the life-history of every parasite at which it could be destroyed, and he held that by destruction of disease germs the tropics could be rendered healthy for the white races.

Two years later he reverted to this theme in an address on "Some Problems in Tropical Epidemiology." The presence or absence of malarial disease or of filariasis in any particular locality, depended, he said, on the presence or absence of their respective subserving mosquitoes in sufficient numbers; the presence or absence of the respective mosquitoes being determined not so much by the presence or absence of the essentials for mosquito life, namely, water, adequate atmospheric conditions, temperature, and vegetable food, as by the presence or absence of special conditions inimical to special kinds of mosquitoes. In 1903, in an address on "The Relation of the Panama Canal to the Introduction of Yellow Fever into Asia," he pointed out that yellow fever might be introduced into Asia as a result of the making and working of the then projected Central American inter-oceanic canal. It was his firm belief that if direct and rapid marine communication with the endemic area of yellow fever were established and if at the same time efficient precautions against the conveyance of the yellow fever germ were not instituted, in a very short time there would be yellow fever in Asia.

In 1908 Manson delivered the Huxley Lecture at Charing Cross Hospital. Appropriately he took for his theme a biological subject. Like Huxley, Manson held that in science hypothesis had a distinct and honourable place, and that it was of the greatest value in stimulating

and guiding observation and in bringing about other discoveries. He then proceeded, with commendable diffidence, to expound his speculations on the origins of parasitic disease.

"That so many parasites require two hosts, a definitive and an intermediary, zoologically so dissimilar as vertebrate and insect or mollusc, has always appeared to me a remarkable phenomenon, and one difficult to explain on Darwinian principles; and yet, undoubtedly, it conforms to these principles. One can readily understand how an animal, originally free, can adapt itself to parasitic life in two absolutely different hosts—hosts so far removed from each other zoologically as a man and a mosquito; and, further, that both man and mosquito are essential for its welfare; and, moreover, that in one host—in this case the vertebrate—the parasite has an asexual life, and in the other host—the insect—a sexual life. How by any recognized principle of evolution can a life so complicated be attained or explained?

"I would with great diffidence, and without entering on detail, submit the following hypothesis. Vertebrates, arthropods, molluscs, helminths, protozoa have all of them sprung from a common ancestor, or, if not from one common ancestor, from similar ancestors. Very early in the growth of the family tree a certain originally free organism became, at first intermittently, then finally and permanently, parasitic on another organism. In time a definite host-and-parasite relationship was established between the two. From the original host, varieties, species, genera, and so on were gradually evolved. Concurrently with this evolution in the host, the parasite, just as its host had done, adapting itself to its changing environment, evolved new varieties, species, genera, etc.

"In the course of these concurring evolutionary processes there came a time when, say, the insect and vertebrate strains began to diverge. A certain organism was parasitic on the common insect-vertebrate ancestor. The process of evolution in this host-ancestor being necessarily very gradual, the parasite —as is the case with many existing parasites—was capable of adapting itself to the closely related varieties of the host, among others to the insect-resulting strain and to the vertebrate-resulting strain. Divergence in these host-strains continuing, varieties became

species, species genera, and so on, until insect and vertebrate, as they exist at the present day, were evolved.

" In the early stages of the evolutionary process the parasite could live in either vertebrate strain or insect strain of host indifferently, but there came a time when it was often more to the advantage of the parasite to pass its sexual life in the ancestral insect strain, its asexual life in the vertebrate strain. In the process of evolution this at first more or less accidental habit—I can hardly apply the expression ' optional habit ' to a parasite—became fixed, and has been transmitted, so that now the sexual life of the malaria parasite is passed in the mosquito, its asexual life in man.

" This is my explanation of the double life, if I may so describe it, of so many and so widely separated parasites, including protozoa, nematodes, trematodes, and cestodes. I can explain it in no other way ; the explanation I offer seems to harmonize the facts and to conform to the doctrine of evolution."

CHAPTER XII

The London School of Tropical Medicine

HAVING attained a position of influence almost unique in his own professional sphere, Manson felt that the time had arrived for the establishment in London of a teaching centre for the comparatively new science of tropical medicine.

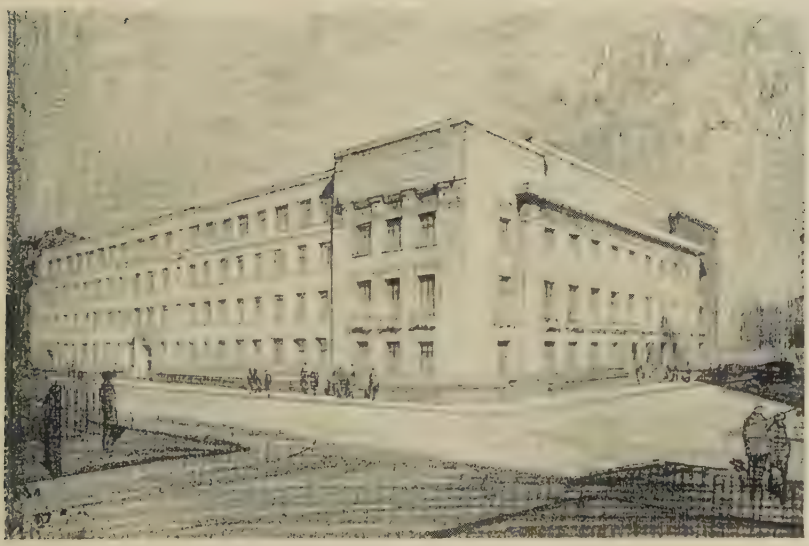
In Amoy he had worked and prosecuted his researches in isolation, far from the centres of scientific thought, and cut off from communion with kindred spirits. In Hongkong he had partially realized his ambition in the establishment of a medical college for the Chinese; but in London his ideas had met so far with but poor response. He could count little on the support of his professional brethren, and even less, he thought, on that of Government authorities, though, as we shall see in the sequel, it was Mr. Joseph Chamberlain who, at the critical moment, afforded him the decisive backing which was essential to the success of his design. He himself, as has been recorded in these pages, had vainly sought more than once for special instruction in tropical medicine. He hungered but was not fed. "I once more came to London on furlough in the year 1882–3," he said,[1] "to learn the latest in medicine and surgery, especially in their application to tropical medicine. I

[1] "Inaugural Address," Society of Tropical Medicine and Hygiene, London, 1907.

heard plenty about the tubercle bacillus, but, although I visited the Societies and became acquainted with many medical men of standing, I did not once hear of Laveran's important discovery of the malaria parasite. Indeed, it was not until I returned to China that I heard about it at all. I read Laveran's first book with great interest, and, as I had abundant material at my command, I set to work to find the organism he described. Had there been a Society (of Tropical Medicine) in 1883, doubtless Laveran's discovery would have been a prominent subject for discussion, the technique for its demonstration would have been familiar to the Fellows, and I should have gone back to China in a satisfactory position to pursue the study under favourable conditions. I lost ten years by this."

Once firmly established in London, he had lost no time in furthering his schemes, and soon other pioneers in tropical medicine were attracted to 21 Queen Anne Street, where they used to forgather in his laboratory to discuss future plans.

Looking back from the position which Tropical Medicine now occupies in the public estimation, it may seem strange that in a country which had been engaged in founding settlements in the tropics for nearly three centuries, the importance of grappling with the causes of tropical diseases should not have forced itself upon the attention of those in authority long before it did. It should be remembered that medical education generally in the mid-Victorian period was defective and inadequate, the requirements of the licensing bodies being so meagre that the description given by Albert Smith of the "Physiology of the London Medical Student" in the process of passing his examinations can hardly be called a caricature. As to tropical disease,



The upper figure shows the original London School of Tropical Medicine at the Royal Albert Dock, with Sir James Michelli, the Secretary of the Seamen's Hospital Society, and (seated) the late Dr. D. C. Rees, the first Director of the School; the lower, the elevation of the new London School of Hygiene and Tropical Medicine, now in course of construction in Gower Street; from the plans of the architects, Messrs. P. Morley Horder, F.R.I.B.A., and Verner O. Rees, A.R.I.B.A.

PLATE XI.

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books of an inferior order, based mainly on hearsay, very little on actual experiment, had been written on that subject, but they were few and their readers limited in number. Even in India, where a well-staffed and well-organized medical service had long existed, it had not been possible to attempt anything serious in the way of systematic instruction in the diseases peculiar to that country. It was still true at the beginning of the twentieth century that medical men went to the Colonies knowing nothing or little of the special diseases which they would have to treat, so that they had to gain their experience at a terrible cost. Even the instruction given at that time at Haslar and Netley was, it is now generally admitted, altogether insufficient. Tropical Medicine, as now understood, did not, in fact, exist.

Such was the position which confronted Manson in 1897 when he began to organize the London School of Tropical Medicine. How he set about the task may be recorded somewhat in detail.

Happening one day in May, 1897, to meet the Secretary of the Seamen's Hospital Society, Mr. (now Sir) James Michelli, in the resident's room of the small hospital at the Albert Dock, he presented him with a scheme, briefly tabulated on half a sheet of notepaper, for the formation in London of a centre for instruction in the diagnosis, treatment and prevention of tropical disease. Manson felt confident that, if facilities were granted by the Seamen's Hospital Society for this object, in connexion with the Albert Dock Hospital, he could enlist the sympathy of Mr. Joseph Chamberlain, the Colonial Secretary. Had it not been for a concatenation of favouring circumstances and the sympathetic attitude of the authorities of the Seamen's Hospital Society, the

seed Manson was anxious to sow might have fallen on barren ground. It did not do so, and soon the germ had developed into a fully matured scheme which he propounded in an address at St. George's Hospital on 1 October, 1897. For seven years in London his had been " a voice crying in the wilderness," but the strongly worded plea which he now made was to meet with due response. His scheme was framed on broad lines. Instruction in Tropical Medicine, he pointed out, was urgently necessary in this country because it was the centre of a great and growing tropical Empire. Rather more than one-fifth of the medical graduates of Great Britain, he said, were practising in warm climates and needed special instruction in the diseases they had there to deal with. At present proficiency in Tropical Medicine was not demanded of the British medical student by examiners, scarcely one of whom had ever practised in warm countries. Moreover, a great many tropical diseases, the signs and symptoms of which were familiar enough to the practitioner in the tropics, were not only unrecognized in England, but were frequently mistaken for other and more familiar clinical conditions. Beriberi was one of such diseases, and filariasis another. As an example of a common source of error, and as illustrating the special technique that was necessary, he mentioned the lack of familiarity with the procedure of searching for the embryo filaria in blood. " If a sailor could command with his eye only the narrow horizon visible from a small boat, he was not likely to see many whales in the sea, so he was wont to ascend into the crow's nest at the masthead and, commanding there a wide view, he could see any whale spouting for miles round. So it was in searching the blood for filaria; a large field was indispensable."

In order to remedy these deficiencies, he urged that a course of lectures on the hygiene and diseases of warm climates should be instituted in each medical school; that a certificate of qualification in these subjects should be granted by licensing bodies, after examination, to those who had attended this course of lectures, and that Government support should be obtained in recognizing the value of men so qualified, by granting them special advantages in Colonial appointments.

Manson further emphasized these points in an address he delivered at the opening of the Section of Tropical Diseases at the Annual Meeting of the British Medical Association at Edinburgh in 1898. There was not one of his hearers, he said, who did not bewail the crass ignorance in which, at the outset of his career, he lightly undertook the care of men's lives in dangerous climates. There was not one of them who did not mourn over innumerable lost opportunities of adding to the common stock of knowledge; opportunities lost, not even recognized, because of the inadequacy and unsuitableness of their initial medical education. There was not one of them who could not pillory himself with the recollection of lives that perished owing entirely to the lack on their part of an elementary knowledge of Tropical Medicine.

The import of Manson's address at St. George's Hospital came in due course to the notice of Mr. Chamberlain. This far-seeing statesman at once appreciated its significance and took prompt action. In a letter to the Board of Management of the Seamen's Hospital Society requesting them to establish a school for the teaching of tropical diseases in connexion with the Seamen's Hospital at the Albert Dock, he stated that he took this step in order that all medical officers, whether serving, or about to serve, in tropical Colonies, should undergo a

course of instruction in tropical disease. The Board of Management of the Seamen's Hospital Society acceded to this request and decided to extend its facilities for study to all medical graduates who desired to make use of them. The Minutes of the Committee of Management of 14 October, 1898, read:

"That a Sub-Committee was appointed, consisting of Mr. H. J. Read[1] of the Colonial Office and others, to formulate a scheme for the organization and management of the new School of Tropical Medicine, with powers to co-opt members of the medical profession interested in tropical diseases and bacteriology."

On 10 March, 1899, after several meetings held at the Colonial Office, and at the residence of Dr. Stephen Mackenzie, the Sub-Committee recommended—

(1) That the name of the School be the London School of Tropical Medicine in connexion with the Seamen's Hospital Society.

(2) That the first teachers in the School and the additional members of the Hon. Medical Staff be appointed by the Committee of Management of the Seamen's Hospital Society in consultation with the Advisory Committee.

(3) That an advertisement be inserted in *The Times* and the medical papers inviting applications for appointments on the Teaching Staff and Hon. Medical Staff of the Hospital.

(4) That there should be three sessions of three months each, yearly.

Thus it came about, with all speed and dispatch, that the modest laboratories of the London School of Tropical Medicine were built (*see* Plate XI), and on 3 October of that year (1899) the first session of the School

[1] Now Sir Herbert Read, Governor of Mauritius.

began, with twenty-seven students in attendance. From January, 1900, the numbers increased to such an extent that further accommodation became necessary, and the School was enlarged from time to time, and a museum, extended library accommodation and other improvements added. At the outbreak of the Great War there had for some years been an average of sixty students in attendance each session. The School itself afforded residential quarters, at first for twelve, later for twenty students.

In assessing the part which Manson played in the foundation of the School of Tropical Medicine and in its subsequent management, some account must be given of the active steps undertaken by Mr. Joseph Chamberlain in his official capacity. The story is best told in a circular which he addressed to the Governors of all the Colonies on 28 May, 1903.[1]

"The great mortality amongst Europeans in such climates as those of the West African Colonies and Protectorate had not failed to attract my notice from the first, as it had that of my predecessors in office, so that towards the end of the year 1897, largely through the interest taken in the matter by Dr. Manson, who had succeeded Sir C. Gage-Brown as Medical Adviser to the Colonial Office, my attention was more definitely directed towards the importance of scientific inquiry into the causes of malaria and of special education in tropical medicine for the Medical Officers of the Crown Colonies. In pursuance of the second of these two objects, it was clearly advisable that a special training school in tropical medicine should be established, where Officers, newly appointed to the medical services of the Colonies and Protectorates, might be given systematic instruction, with special facilities for

[1] Colonies: Miscellaneous Paper relating to the Investigation of Malaria and other Tropical Diseases and the Establishment of Schools of Tropical Medicine. Presented to both Houses of Parliament by Command of His Majesty, June, 1903.

clinical study, before leaving England to take up their appointments, and where doctors already in the Service might, when on leave, have opportunities of bringing their professional knowledge up to date; that all leading medical schools in the United Kingdom should be invited to give greater prominence than hitherto in their schemes to the study of tropical medicine; that the medical reports periodically sent from the tropical Colonies and Protectorates should be re-cast on one uniform type designed to throw light on the diseases most prevalent in tropical countries, and to indicate the methods likely to be most successful in preventing or curing such diseases. With reference to the first of these objects, the provision of a special training school, it was considered that the Albert Dock branch of the Seamen's Hospital, which was about to be enlarged, was likely to offer the facilities required, standing as it does at the Dock gates, admitting sufferers from tropical disease direct from ships from all parts of the world, and being, moreover, within easy reach of the Colonial Office, and therefore likely to be in close touch with that Office. These considerations decided me to approach the managers of the hospital in preference to making whatever arrangements might be possible for obtaining the object in view at the Royal Naval Hospital at Haslar, or the Royal Victoria Hospital for Soldiers at Netley, at which latter hospital officers of the I.M.S. receive instruction in tropical diseases."

Mr. Chamberlain caused a letter to be addressed to the General Medical Council and the leading Medical Schools in the United Kingdom, pointing out the importance of ensuring that all Medical Officers selected for appointments in the tropics should enter their careers with expert knowledge for dealing with such diseases as prevailed in their region. Most of the replies received were sympathetic, and the correspondence showed that at upwards of twelve medical schools, some of them situated at large seaports, arrangements were about to be made for giving special instruction in Tropical Medicine. The General Medical Council adopted the following resolution:

"That while the Council is not prepared to recommend that tropical medicine should be made an obligatory subject of the ordinary medical curriculum, it deems it highly desirable, in the public interest, that arrangements should be forthwith made by the Government for special instruction in tropical medicine, hygiene and climatology for duly qualified medical practitioners who are selected for the Colonial Medical Service and who otherwise propose to practise in tropical countries."

Shortly after the inauguration of the first session of the School, Mr. Chamberlain (11 November, 1899), wrote:

"It is clear that the work cannot stand still, and as long as those who can speak with the authority of science are confident that by human effort the rate of mortality from malaria and other tropical diseases can be greatly reduced and the strength and efficiency of Europeans resident in unhealthy climates can be sensibly increased, so long, in my opinion, ought funds to be forthcoming for carrying on what has been so well begun."

The establishment of a separate branch of medicine and of an educational centre for its teaching were not effected (as it is hardly conceivable that they could have been) without opposition, both from within and without the profession. Pioneers in discovery and in education have attained their ends only by facing opposition and official discouragement; and Manson was not to be deterred by those who were covertly or actively hostile to this new departure. Many were the protests from his professional brethren; some saw in him a follower of Cagliostro; others contended that the existing knowledge of tropical disease sufficed for all practical purposes.

In reply to an address on a School of Tropical Medicine given by Manson in April, 1900, to the Colonial Institute, the then President of the Royal College of Physicians considered himself justified in stating that "the study of tropical disease is not so entirely distinct from the study

of disease generally as Dr. Manson's address might lead one to think. The methods of investigation, carried on in the pathological laboratories of every properly organized medical school for detecting morbid conditions of the blood, are identically the same, though not quite the same stains are used, as the method of investigation for diseases which are malarious ; and in the same way the dependence of disease upon parasites, external and internal, is also recognized in this country too." This in itself was a misleading commentary from an eminent physician on the speech of a colleague which had been generous in its breadth and moving in its appeal. Manson had pleaded for more than the official recognition of his school : he had pleaded for the recognition of Tropical Medicine as an Imperial necessity and as essential to the future prosperity of the Empire as a whole. This broad survey is best expressed in his own words :

" A striking feature in modern civilization is the attention that is now bestowed on public health. The individual man has always been sufficiently solicitous about his own health, but it is only recently that he has taken seriously to care about the health of his neighbour. Not that nowadays we really care more for our neighbours than our ancestors did ; we do so only, or principally, because science and experience have taught us that if our neighbour fall sick, sooner or later, and in consequence of this, we ourselves suffer in body or estate.

" The position of influence and power which is gradually being conceded to my profession has been attained through no violent bloodstained revolution or cunning intrigue, but has come to it purely in virtue of the public's appreciation of the public's interests, the recognition by the public that the physician is one of its most valuable servants.

" Perforce, in order to overtake the studies and perform efficiently the duties, the medical profession has had to subdivide, one set of men devoting themselves to one department of pro-

fessional knowledge and practice, another to another—specializing, as it is called.

"Climate is a potent factor in the grouping and distribution of animals and plants. It has an equally potent influence on the grouping of diseases which, as modern science has amply shown, are, in the great majority of instances, but the effect of animals or plants parasitic in, or on, or near the human body.

"As yet the school is in its infancy and requires fostering; but I make bold to say that it will thrive, and that it will do more for our Tropical Colonies than any measure yet devised for their benefit. That is a strong statement, not too strong, however, in my opinion.

"This school strikes, and strikes effectively, at the root of the principal difficulty of most of these Colonies—disease. It will cheapen government and make it more efficient. It will encourage and cheapen commercial enterprise. It will conciliate and foster the native. Think for a moment what would be the result were we to banish malaria from such a place as the Gold Coast. Why, the Colony would go forward in prosperity by leaps and bounds. 'Banish malaria!' you will say. 'Impossible!' Not so. I have shown you how it has been done, and how it can be done. I have lived for long periods, thirteen years in one instance, six in another, in what, at one time, were regarded as two of the many so-called 'white men's graves'—Amoy and Hongkong. During one of our early Chinese wars a British regiment was stationed on a little island in the harbour of Amoy. In this same island, many years later, I had my home. The regiment I refer to was nearly wiped out by malaria and other tropical diseases, as the tombstones in the little graveyard bear witness to this day. But in all my thirteen years in Amoy I had only one slight attack of fever, and none of my family suffered in that way. In Hongkong in the early forties, and for a good many years after its occupation by the British, the annual mortality among our troops from malaria and other tropical diseases was from 25 to 30 per cent., some years even more. Yet when I lived in Hongkong in the eighties, I never had an attack of malaria, nor had any of my family, and the total mortality from all causes among the garrison was something like 5 per 1,000 only. The terrible mortality of the early years of British occupation of Amoy and Hongkong arose from ignorance. The comparative salubrity of later years

arose from experience; and now that we can supplement experience by knowledge, why should we despair of such places as the Gold Coast, for example, or of any part of Tropical Africa or other fever-stricken land?

"Our tropical schools must not be starved for want of funds. They are of national importance. They are at the root of tropical sanitation. It would be a disgrace, and a piece of parsimonious folly, if, for the sake of a few pounds, these promising institutions were allowed to languish and collapse."

There was opposition, amounting almost to definite hostility, from others highly placed in the profession, and even from some members of the staff of his own hospital, and letters of frank enmity appeared in *The Times* (December, 1898). Nor did the already existing colleges of medicine in London appear to be entirely sympathetic. The correspondence was forwarded to Mr. Chamberlain and elicited a characteristic reply to Manson:

"Mr. Chamberlain has complete confidence in your advice with regard to the School, and he does not want to be drawn into a professional dispute, which he feels sure will not interest the public. He doubts if the opposition will in any way injure the hospital, but, if you wish it, he is ready to offer to hear a small deputation to discuss the matter with you."

That there was no need for any such measures, we can now record; the short-sighted policy of some was well countered by the testimony of those who had themselves been through the mill in the tropics. Dr. S. R. Hodge, the Senior Surgeon to the Wesleyan Mission Hospital, Hankow, China, in a letter to *The Times*, wrote:

"I feel sure that the medical profession will be true to its great ideals and treat the question on its merits. Already many of us have to go abroad for special instruction in special branches, and

it will be a national disgrace if the greatest colonizing Empire of the world has to send its medical men elsewhere for their tropical training."

In order to meet the cost of the buildings and suitable equipment, Mr. Chamberlain presided at two dinners at the Hotel Cecil, the first on 10 May, 1899, the second about the same date in 1905, and approximately £12,000 was raised on each occasion. In 1912, in memory of his father, and in recognition of the work that the School had done, Mr. (now Sir) Austen Chamberlain appealed for more funds and some £73,000 was received, which was appropriately divided between the needs of the upkeep of the hospital, the provision of new apparatus, and the prosecution of research. The initial grant from the Government had been a comparatively small sum of £3,550, and it is gratifying to know that financial stability was to a great extent maintained by the students' fees. In 1920 the School received further support; thanks to the good offices of Lord Milner, a special fund of £100,000 was raised, part of which was devoted to permanent endowment, the remainder being utilized for removal of the School from its former situation at the Albert Dock to its present position in Endsleigh Gardens, in the University quarter of London. This move was made in January, 1920, and there the School continued to flourish for the two remaining years of Manson's life. In its affairs he never ceased to take the keenest interest, and shortly before his death he gave much thought to the plans for the incorporation of the School with the London School of Hygiene, a union made possible by the munificent Rockefeller gift of two million dollars, and soon afterwards consummated.

The housing of the School and the Hospital under one roof has obvious advantages for the teaching of Tropical

Medicine. Here, as at the Albert Dock, but more fully, Manson was able to see the practical realization of his schemes for the co-ordination of the clinical and laboratory elements of Tropical Medicine.

In 1920 it was stated that, since the School's foundation, twenty years previously, a quarter of a million pounds had been raised for its maintenance. At the time of Manson's death (1922), no fewer than 2,544 students had passed through the course, and the greater number had taken the School's certificate of proficiency. Manson's course of teaching was not only taken as a model by other Schools of Tropical Medicine which sprang up in various parts of the world, but led in 1904 to the creation of a Diploma in Tropical Medicine by the University of Cambridge, in the institution and conduct of which he played a prominent part;[1] and from that time onwards other Universities and Colleges have followed suit.

The School of Tropical Medicine in London, and the sister institution in Liverpool, which was founded about the same time, became the models for similar institutions in various foreign countries. In France Schools were started in Paris, Bordeaux and Marseilles. An *Institut für Schiffs- und Tropenkrankheiten* was founded in 1900 at Hamburg, and in Belgium a State School was established in 1901. Other Schools have since sprung up in India, Italy and the United States.

When the proposal to establish a School of Tropical

[1] "A special Diploma from a University would, I believe, tend to foster and advance the study of Tropical Medicine. But the qualifying standard should be a high one. It should imply not only a good practitioner, but a capable investigator."—Extract from Manson's letter to Special Board for Medicine, Cambridge University, dated 4 November, 1903.

Medicine was first mooted, the Hospital at the Albert Dock was a small building with accommodation for twenty-five patients. Thanks to the generosity of the Seamen's Hospital Society, the number of beds was doubled and the Hospital considerably enlarged, in order to provide an adequate number of clinical cases for teaching and research. At the beginning, and all the way through, the support rendered to the scheme by the Society was invaluable; and it is hardly too much to say that if Manson was the father of Tropical Medicine, the Seamen's Hospital Society was its nurse, so influential has it always been in fostering this branch of Medicine.

The following description of Manson as a teacher is from the pen of one of his most brilliant pupils, Dr. H. M. Hanschell: "His advent at the Albert Dock Hospital was always the signal for an eager move from laboratory to wards. An afternoon with him in the broad wards of the hospital was a memorable experience. In common with all great clinicians and teachers, he never needed to select for demonstration 'good,' which usually means 'obvious,' cases. He took cases and opportunities just as they came, and his examination of them would disclose in due order and with sufficient precision all detectable features from which inferences might justly be drawn. Manson knew men and cities and things. His wide experience, his reading, his sympathetic observation of human beings of all races, were brought in aid to illuminate a problem and give point to a conclusion. No wonder that his usual hour and a half of bedside teaching found him always surrounded by an enthusiastic class whose very stillness masked their interest and attention. He was benignant of aspect and kindly in manner, and, even when in pain,

hobbling stick-aided, or wheeled in a chair, from bed to bed, patient, courteous and untiring. He rebuked the slack and indifferent effectually enough by ignoring them. His sympathy with and interest in the keen were obvious.

"Two traits in Manson his house physicians came to know well. One was his special interest in worm and particularly filarial diseases; the other, his affection for his Chinese patients. Smilingly and hopefully he would address queer noises to them, usually to be disappointed that they did not comprehend his particular brand of Chinese. To young medical men about to leave for practice in the tropics he would speak quietly and wistfully of the great chances of adding to the common stock of knowledge that would surely come their way. Clearly he envied them. To those among them doubtful as to the future and its material rewards, he would say firmly: 'You must cast your bread upon the waters, that it may come back to you after many days.'

"Manson was judicious in his praise; it was therefore worth receiving. He would notice and commend careful work, not only in medical subordinates, but in sisters and laboratory assistants—those faithful non-commissioned officers in the great Medical Service. Was it not a Chinese laboratory assistant who found the first filaria for him? In wards and lecture theatre his discourse was clear, precise and to the point. It was always informal, with a winning dignified kind of intimacy and friendliness. Well flavoured with Attic salt, a grave sincerity and seriousness of purpose sustained it all. The infirmity of body and the drive and clearness of mind were ill-matched, yet the last seemed to gain in effectiveness from the first. And best trait of all in Manson's character was his readiness to listen

for any length of time to the theories or queries of any disciple whom he deemed sincere. It is sometimes said of a man, as if in praise, that 'he did not suffer fools gladly,' and often this means that he suffered not those who disagreed with him. This was never true of Manson. He looked only for *sincerity*, and for this suffered patiently many who had more zeal than discretion."

The convictions ultimately formed by Manson regarding the organization and conduct of a School of Tropical Medicine, as the result of his considerable experience of the working of the School which he founded, were set out by him in the year 1905, and at the present juncture it is not inopportune to recall them. The following schema may therefore be quoted:[1]

(*a*) The School must be in close association with a hospital where tropical cases are plentiful, or can be concentrated.

(*b*) The laboratory and class-rooms should be so near to the wards that the student can have immediate access to any patient whose disease he is engaged in studying.

(*c*) The Staff should consist of:

1. A superintendent demonstrator or tutor, who should have the run of the hospital cases and be director of studies as well as general demonstrator. The salary of this officer must be a good and rising one, so as to secure high ability and also (and this is of importance) continuity in office.
2. Assistant demonstrators.
3. Lecturers, physicians and surgeons who themselves have had tropical experience.

(*d*) The length of the course of study should be not less than three months, and the entire time of the student must be devoted to the work.

(*e*) The Certificate of the School should be given only to those who have passed a satisfactory examination.

(*f*) There should be special chairs in protozoology, helminth-

[1] Lane Lectures, August, 1905.

ology and entomology, subjects inadequately taught in the medical schools, but of great importance in tropical medicine.

(*g*) There should be a research department, a museum, and a library.

(*h*) The School should be open to post-graduates only.

(*i*) To meet the conveniences and exigencies of medical men returning from or going abroad—and this is a practical point of considerable importance—there should be three complete sessions annually.

(*j*) The superintendent, the protozoologist, entomologist and helminthologist should give their whole time to the School and to research.

At first it might not be necessary to employ so large a staff as indicated; it might be prudent to begin on a more modest scale. In undertaking such a school, plans should be laid on a broad basis, so that when expansion becomes necessary, pulling down and reconstructing, always an expensive proceeding, may not be required.

In the course of the Great War the pupils of the School were scattered over the four quarters of the globe. Wherever Fate conducted them, they bore with them the lantern of knowledge; for the armies on the Eastern fronts being exposed to more danger from the scourge of tropical diseases than from the bullets of the enemy, there arose an insistent demand for doctors who, having been specially trained in the diagnosis and treatment of tropical diseases, understood how to combat the malaria, dysentery, relapsing fever, and other epidemics which loom so large in the Medical History of the War. Thus, at a crisis in the history of civilization, Manson lived to see his Foundation vindicated and its claims for the means of increase and expansion generously recognized.

CHAPTER XIII

The Closing Scenes

RECURRING attacks of gout and the inroads of age had not impaired the vigour of Manson's mind, but had seriously undermined his physical strength, so in July, 1912, he decided to relinquish his arduous duties at the Colonial Office.

For the services he had rendered the State he was awarded a signal honour in the G.C.M.G., and he received what was perhaps equally grateful to him, many testimonials of regard from the officials of the great Department with which he had been so long connected. Mr. Lewis (afterwards Viscount) Harcourt, the Secretary of State for the Colonies of that day, expressed "high appreciation and deep gratitude for all you have done for the Department and for the cause of Tropical Medicine."

No sooner was he released from harness than he left London, and in November of the same year, after an absence of twenty-three years, revisited the tropics, spending four months in Ceylon. New scenes and customs always interested him; he was enthusiastic about ancient monuments, jungle life and native customs; but he did not omit to seize every opportunity of seeing new cases of tropical disease.

Returning to England in March, 1913, he passed the summer in Ireland. In the autumn he travelled to

South Africa and Rhodesia, visiting the Victoria Falls, and interesting himself keenly in scientific investigation and in medical affairs generally. Once more in England, in August of that year he became the central figure of the Tropical Section of the International Congress of Medicine in London. This was made the occasion of an address by Professor Blanchard,[1] in which he was hailed as the "Father of Tropical Medicine" and presented with a gold medal (executed by Dr. P. Richer, of Paris) which had been subscribed for by a specially convened International Committee.

In 1914, full of honours, Manson retired to a property he had acquired on Lough Mask, County Galway, where he had erected a picturesque fishing lodge which he christened "The Sheiling." His garden was laid out on quite original lines; the pathways were constructed on the pattern of grouse-claws, and lined with stones of varied shape and hue. He was almost as keen a gardener as a fisherman, finding delight, as he said, in "seeing things grow."

Manson soon became a familiar figure in that part of Ireland; the people would commonly refer to him as "Saint Patrick," and he never quite succeeded in disabusing them of the notion that he was an Irishman. His fame as a doctor spread far and wide in that locality, and his aid was frequently sought by the peasants, who would often travel many miles to see him. There were

[1] Raphael Blanchard, the eminent parasitologist, became a member of the Académie de Médecine at 37 years of age, and had been Professor in the University of Paris since 1883. He was one of Manson's closest associates and always his great admirer, frequently referring to him as "l'homme que je regarde comme mon père." He died in 1919, and one of Manson's last contributions to the medical press was an account of his life.

occasions, too, when, responding to earnest entreaties, he would undertake a prolonged journey to visit the sick in some hillside bothy. Once, in June, 1920, when crippled with gout, he was hoisted on to a pony and rode some seven miles across the hills on such an errand.

When the Great War came he regarded the catastrophe in a philosophical spirit and cherished no racial bitterness. He was amused at the perturbations of those around him. Thus on 29 September, 1914, we find him writing: "Here we live from post to post wondering how the war is progressing. It is very unsettling. Michael [1] sees aeroplanes in every cloud-fleck, even in soaring hawks, and Tom Joyce [2] sees German spies in every tinker on the road, and hears bombs in the shots in distant quarries. Amelia [3] don't like them aliens, and the cook is in such a funk about her sweetheart, who is on one of them 'men o' war,' that she forgets to boil the onions enough. The fishing has gone to the devil, too—Michael thinks they suspect him of being a German."

Old and infirm as he was, he considered it his duty to offer to the War Office his special knowledge and experience for any useful work that could be assigned to him. It was his special request—which could not be favourably considered—that he might be sent to Egypt, and about this time he wrote: "I wish I were a young fellow again and working with you and the other fellows in Egypt who are creditably representing there the Tropical School."

Once during the war he was called upon to exercise his professional skill, and, travelling to the

[1] His Fidus Achates and head boatman.
[2] Second boatman. [3] His faithful maid.

South of France in 1917, he narrowly missed being torpedoed.

One of the last honours he received was the honorary degree of LL.D., conferred upon him by the University of Cambridge on 18 February, 1921. The University orator celebrated his praises in words that are worthy of record:

"Quibus morbis longa per saecula quot Anglos lugemus absumptos, dum imperium nostrum apud Indos, apud Afros, provehunt. Sed nostris temporibus facilius depelluntur, naturâ melius cognitâ. Quantum in his studiis profecerit hospes noster, non ille Dossennus edacibus in parasitis, non meum sit scientibus exponere."

During the summer of 1921 Manson suffered from alarming heart attacks. The first of these occurred while he was fishing on Donside in Aberdeenshire, in June; nevertheless, as soon as the political situation in Ireland had become clearer, he insisted on travelling once more to The Sheiling. While crossing the Irish Channel he became critically ill, and to his anxious attendants it seemed hardly possible that he could reach his journey's end, but his unbending will carried him through. During the remaining months of the year he had become so incapacitated and enfeebled that he could no longer hold a fishing-rod, but he nevertheless persisted in going out on the lake. Owing to the political turmoil in Ireland, which he did not entirely escape, he retired in November to his flat in London. Though sick in body, his mind remained clear, and once more he busied himself in thinking out new lines of research, especially in planning an expedition to Samoa then being organized by the School of Tropical Medicine.

His last appearance in public was on 20 January,

PLATE XII.—Sir Patrick Manson at the age of seventy-four, with his grandsons.

20 JUN 27

1922, when he was presented with a portrait, subscribed by a large number of students of the London Tropical School and sympathetic friends. An announcement followed that money was also forthcoming for a medal to commemorate his work, and this is now awarded biennially by the Royal Society of Tropical Medicine and Hygiene to the living author of such original work as the Council may consider to be deserving of such a distinction.

Fourteen days before his death he again visited the School and critically examined specimens illustrating the pathology of filariasis. On 7 April he realized that the end was approaching, and calling for pencil and paper he wrote steadily on till strength failed him. The following sentences have with difficulty been deciphered from the script :—" I propose to found a Manson Scholarship. Candidates must see to it that in their application they specify what line of investigation they propose to follow, and where their research is to be carried out, advantage being taken of existing laboratory facilities. The scholar thus chosen will be expected to carry out the proposed investigation up to a point at which it can be announced that it is likely to be fruitful; but if it is not likely to be promising, no objection shall be raised to changing the subject, methods or execution." Finally, his mind reverting to his School, he wrote: " We are doing good work, both in teaching and in research, and we are paying our way." The authors of this biography cite these passages in the hope that no difficulty may be found in connecting the scholarship with the new London School of Hygiene and Tropical Medicine.

Manson's mind in these last days lost nothing of its clarity, and in bidding good-bye to those around him

and thanking them for their attentions, he quoted some lines from Kingsley's poem, *The Invitation*:

> "Do the work that's nearest,
> Though it's dull at whiles,
> Helping when we meet them
> Lame dogs over stiles."

So, on 9 April, 1922, in his seventy-eighth year, he breathed his last.

A memorial service was held in St. Paul's Cathedral on 12 April, and on the following day Manson was laid to rest in Allenvale Cemetery, Aberdeen, in a picturesque spot he himself had selected a few months before, commanding a fine view of the bend of the Dee where fishermen cast their nets.

By a cruel irony of fate, immediately after the news of his death had filtered through to Ireland, his fishing lodge was raided and many much-valued personal relics were destroyed.

Manson had outlived most of his colleagues and contemporaries. Some of those with whom he had been most intimately associated in his scientific work he had never met personally. Among them was General W. C. Gorgas, of Panama fame, Director of the American Army Medical Services, who predeceased him by two years. When Gorgas died in 1920, and received the signal honour of a military funeral in London, it was noted that the only wreath resting on the coffin as it was borne through the cathedral was one sent by Manson. "The work my husband accomplished in yellow fever and malaria," wrote the General's widow to Lady Manson, "was founded on the discoveries of your husband, Sir Patrick Manson. The world will not forget him, and the benefits of his work every generation

will know and appreciate. Dr. Gorgas yielded to no man in his love and admiration for Sir Patrick."[1]

With none of the aids which wealth and position can bestow, Manson had carved out for himself a career of great distinction, and had created a new department in medicine. That he did great things for suffering humanity and for civilization, no one will gainsay, and it may safely be said that of all his medical contemporaries there are few whose work will endure longer than his. If he has not always received the credit which was his due, the explanation is largely to be found in his dislike of anything that savoured of personal advertisement. When personally attacked, he always preferred to be silent, leaving the facts to speak for themselves.

It is too soon—even if it were quite within our province—to attempt to place Manson in his historical perspective. Many good men regard him as the Father of Tropical Medicine, meaning thereby, not that he was the first to study the prevalent diseases of tropical countries, but that he was the first to expound by simple, verifiable, experimental methods the manner of their causation and propagation. Here we may further claim for him that by his discoveries he gave timely and very necessary point and impetus to the venerable but somewhat neglected truth that Medicine, as a science, has its living roots in Biology, and so led medical education back to those solid biological foundations from which it has been inclined to stray. He had gone

[1] It was General Gorgas who, applying the discoveries of Manson and Ross in malaria, and of Reed and his co-workers in yellow fever, stamped out these scourges in Havana, and who by his anti-malarial measures made possible the construction of the Panama Canal. His was a very attractive personality.

forth to China, and from thence had taught a new doctrine of parasitology; returning to the West, he had taught the medical world new and fruitful lines of research, and in doing so had reminded our educationists of the vitality of the ancient ties between medical science and biology.

Greatly distinguished in his career, absorbed in many fascinating problems of medicine, honoured in the School which he founded, appreciative of sport and good literature, fortunate in his home life, and a lover of children and animals, Manson's was a full and happy life, and though much of his happiness came to him unsought while pursuing great and disinterested aims, he belongs to the company of those who " warmed both hands before the fire of life."

CHAPTER XIV

Manson's Philosophy of Life

In tracing the career of one who was an outstanding personality we have emphasized the more conspicuous features exhibited in his work and public duties; but there were other qualities which must be noted. He possessed singular charm of manner, which was not unconnected with his faculty of being able to grasp a point of view different from his own; these attributes not only won for him many friends but enabled him to enlist the loyal co-operation of others in his designs and stood him in good stead in the committees over which he presided or on which he served during the years of his life in London. Capable of discovering merit where others failed to see it, he fired the enthusiasm and imagination of his junior colleagues, and both by example and by precept brought home to them the great desirability of undertaking original research. So long as his pupils showed promise and were willing to apply themselves to the work in hand, he was tolerant of personal peculiarities or shortcomings. To a critic who expressed disapproval of the views of a younger colleague his decisive reply was: "That young man has brains and ability, so make the best of him."

Manson's innate courtesy and kindliness contributed to his success both as a practitioner and a consultant. Though himself ailing during his declining days, so

that any exertion became a great labour, he never failed in sympathy and in solicitude for the welfare of others. He would insist, for instance, in spite of pain and disability, on visiting friends who happened to be ill.

In fields of knowledge outside the realm of medicine he was uncommonly well informed, for he had great capacity for absorbing and retaining information on all kinds of topics. To his retentive memory he owed not a little of his success in public life. He seldom forgot what he had once read, and passages from the classics which he had learnt in early life came readily to his tongue. His power of sustained concentration was remarkable. When intent upon a new theory he would sit for hours completely detached from all that was going on around him, but when the formulation was complete he was able to dismiss it from his mind and concentrate with the same intensity upon the work that came next to his hand. "Learn to think," was a favourite injunction of his; "most people do not think at all!" He had acquired the habit of thinking at odd moments, and would often say that he had done his best work during his morning bath. In his reasoning he used the two logical methods with equal ease and freedom, the exactitude of his inductions being equalled, and sometimes eclipsed, by the intrepid sagacity with which he applied them to prolific deductive purpose. Sometimes his deductions might seem to savour of intuition, but they were really hypotheses based on ascertained facts, not unaided by a sense of analogy. He had had his early training in the land where deductive reasoning prevails, but, as in the case of the great John Hunter, his compatriot, the method which corrected his mental operations was that of induction.

One principle on which his researches were conducted,

and the standard he set and expected from others, he epitomized as follows: "Never refuse to see what you do not want to see, or what might go against your own cherished hypotheses, or against the views of authorities. These are just the clues to follow up, as is also, and emphatically so, the thing you have never seen or heard of before. The thing you cannot get a pigeon-hole for is the finger-point showing the way to discovery."

With him the love of poetry was an absorbing passion. During his busiest days he found time, even while shaving, to commit lines to memory, and he was fond of quoting from his favourite authors—Burns, Shakespeare, Tennyson and Kingsley. "I am very pleased you have taken to poetry," he once wrote. "Few things have given me so much pleasure. When I am lonely and worried I have only to repeat some poem to myself and I have company and music and my troubles often fly away. It never goes back on you. Learn as many pieces as you can. Go over them again and again till the words come of themselves, and then you have a joy for ever which cannot be stolen or broken, or lost. This is much better than diamond rings on every finger."

As a public speaker Manson was always informative, and often witty, and his delivery was good. "I wish I had had the benefit of an early training at a Union," he wrote. "In practising speaking, the successful dodge is to try not to say too much, but to have one or two big points and to rub them in thoroughly. Also never lose your temper, or be too dogmatic, or in any way discourteous."

His keen sense of humour and his fund of good stories lightened his labours, and made him one of

the best after-dinner speakers of his day. But he felt himself most at home, and was in his gayest mood, among his folk in Aberdeenshire, when, at some Highland gathering, with keepers, gillies and other retainers as his audience, he would tell a tale in the native Doric with great gusto and acceptability.

Once he was called to a northern town in consultation to a patient of some consequence who had been in the hands of other physicians; the case was a complicated one and necessitated several visits. The patient appears to have assumed that no fee would be expected, but long afterwards sent him a book of poems containing the inscription: "To one who succeeded where others failed." Manson neatly modified the inscription so that it read: "To one who failed where others succeeded."

In his dealings with men, Manson usually showed a nice discrimination. In proof of his ability to choose the right man for the right place, it is only necessary to recall the fact that the two untried young men whom he selected five-and-twenty years ago for the new specialities in Tropical Medicine, helminthology and protozoology, were Professor R. T. Leiper and Dr. C. M. Wenyon.

Though vigorous and robust in frame, he was an almost lifelong sufferer from gout, as the reader will have gathered. That painful and disabling malady first assailed him at the age of twenty-seven, and his activities were crippled by it for the remainder of his life. Year by year he had attacks of variable duration, and about once in every three years it would confine him to bed for some six weeks at a time. A curious manifestation of his "goutiness," and one which, at times, became almost intolerable, was the itching of the feet at night

to which Strabo refers as "the lispings of the gout." Let us hope that, like Sydenham, the English Hippocrates, also a lifelong sufferer from gout, he was able to console himself with the reflection that "more wise men than fools are victims of this affection." But the loss of time and opportunity which these frequent illnesses entailed was harder to bear than the acute pain. Here is his description of one of the attacks:

"You must think me a very poor correspondent but, unfortunately for me, I can plead too good an excuse. To-day, for the first time, I have been able to stand on my own feet. You have no idea how circumscribed this inability to walk makes one's activities. I have to ask for everything—can't get a book until I get some one to hand it over to me. Fortunately, I can sit in a boat and catch salmon, and I have persisted in going out daily and taking fishing exercise. Consequently my general health has not suffered much, and I have not unremedially stiffened. So I hope some day soon to get to work again."

Again: "I had a nasty attack of gout, which got on my right hypoglossal nerve, and at the same time apparently aggravated the weakness which I have had for years in my right hand, so that letter-writing, or any writing, became to me a grievous labour and generally resulted in incomprehensible hieroglyphics. I had to wait therefore till I got back to my typewriter before attempting to reply to a lot of letters. It was a curious attack I had. I had been laid up for one day with fairly sharp gout. Next day the pain in my feet had gone, but I had sharp wandering pains all over my body and joints and fasciæ. That night I felt that my tongue was too large for my mouth, and next morning I was deaf on the right side, had bad pain in the right ear, and on looking at the tongue found it on protrusion deflected to the right side and the muscle on that side soft and flabby. I saw Ferrier a few days later, and he told me he considered that I had a small embolus at the root of the hypoglossal. I am not sure about the correctness of his diagnosis, as it does not account for the deafness in the ear. However, it cannot be helped. The pain soon disappeared, and the deafness too, and the tongue is distinctly better, though not quite right. My right hand is more wasted and, for fine work,

weaker than before, but my general health is first-class, better than it has been for years."

Of Manson's prowess with the gun we have already had something to say. As his walking powers became more limited, he was restricted to shooting from a butt, but as late as his sixty-fifth year his aim was unerring. The favourite sport of his old age was trout-fishing with a dry or wet fly. He was particularly fitted by temperament to be an angler, and he was never in so happy a mood as when for many hours a day he was casting over the still waters of some favourite lough. When prevented by increasing infirmities from wading, he took to spinning or " dapping " from a boat. On these excursions he was usually accompanied by one of his younger friends. He preferred their company to that of older friends, for he himself remained youthful in ideas and outlook, and in his undisguised zest for life. It was easy to see in him on these expeditions many of the attributes of the true naturalist: he was observant of all around him, and the formation of a cloud, the contour of the rocks, the effect of weathering on different geological strata (for he was no mean geologist), the behaviour of birds, the effects of light and shade upon the still waters, were all alike phenomena in which he delighted. Fishing was with him a science, and he angled for fish as thoroughly as he had hunted for parasites. When it came to hooking a fine fish he had a keen eye and a quick wrist. On the dreariest and dullest of fishing days his interest never waned, and he usually claimed to have enjoyed himself to the full, though his creel might be empty. The size of the catch may be taken as the mark of a fisherman, and he was usually able to make a good response to the test. In May, 1909, he landed a record trout of 11½ lb. from

Lough Mask, and in 1911 he secured on Corrib (the neighbouring lough) eighteen brown trout weighing 35 lb. on the dap and wet fly. From 1913 onwards to within a few months before his death he spent the summer months on his Irish estate absorbed in his favourite sport, but in 1921, owing to the turmoil in Ireland, he revisited the fishing haunts of his boyhood on the Don, and it was there, as we have mentioned, that he suffered the first of the heart attacks to which he finally succumbed.

As we have seen, not even gout could keep him from his favourite pursuit. Writing on 13 June, 1920, he said: "I can only hobble about a few yards on a couple of sticks, and then only with much suffering and effort. I can fish all right from the boat, but have to be wheeled down from the jetty and be almost lifted out and in from the boat." In order to visit the more distant parts of Lough Mask he availed himself of a motor-boat, which interested him all the more from its liability to breakdowns. In this rather cumbersome craft he was able to explore new ground and to point out to visitors numerous beauty-spots. An al fresco luncheon on some rocky islet was a feature of these outings. Here with his thermos flask and the inevitable black bag which accompanied him, he was in the best of spirits, and his enjoyment of the frugal repast seemed to be enhanced by the presence of the biting flies in which these islands abounded. They seemed to remind him of his interest in tropical pathology, but they were regarded less indulgently by his guests.

As a clinician, Manson's judgment was essentially sound; he made singularly few mistakes, and it was this faculty of judgment, the result of keen observation and wide experience, that made him an ideal consultant,

even in diseases which were outside the usual scope of his practice.

As an example of his skill in divining the life-history of parasites which has not yet been mentioned, we may cite his working-out, in the second edition of his "Manual of Tropical Diseases" (1898), of the life-history of *Schistosoma hæmatobium* outside the human body in a hypothetical molluscan host. This has been justified by later discovery. Another instance, which might seem almost uncanny, came under the writer's notice. In 1919 he was examining a case of *Filaria loa* infection from West Africa. The patient himself had already noticed the presence of the parasites travelling under his skin, but on examination of his blood, no embryo filariæ were visible, whereupon Manson remarked that the embryos would not appear till six years had elapsed from the time of the original infection (1916), and that if the blood were searched in October, 1922, they would be found. A careful record was kept and in October, 1922 (six months after Manson's death), embryos were for the first time found in the blood.

An example of his resource in practice may be related. A patient of his in Hongkong had already had one kidney removed for stone and was maintaining a precarious existence on the other, when it too became the site of abscess-formation, for which immediate operation had to be performed. The patient was left with a persistent sinus in his side from which poured pus. Suddenly, after a severe bout of pain, complete suppression of urine took place, indicating that the second kidney had gone out of action. Hurriedly summoned to the ward, Manson seized a catheter, cautiously introduced it into the sinus, and, coming upon something

hard and solid in the pelvis of the kidney, continued to exert gentle pressure, pushing the catheter down the ureter, and thus succeeded in dislodging a calculus that had become impacted there, forcing it into the bladder and so saving the patient's life. Of his skill in locating liver abscess, an instance may be cited from his practice in London. A patient, a distinguished peer, was being operated upon for this trouble by a leading surgeon, who was assisted by two other surgeons. Their repeated efforts to locate the abscess failed, and, after a dozen or so unsuccessful punctures had been made into the liver, Manson, who was the physician in attendance, was asked to try his hand. Driving the needle through the chest-wall in the direction suggested by his experience of such cases, he was able to tap the abscess cavity at the first attempt.

Manson seldom referred to his early researches, but when speaking about his malaria work in his last days, he said: "In 1878 I knew enough about microscopic appearances of the blood to have recognized a malarial crescent had I seen one. I should then have been able to anticipate Laveran by two years. Luck was against me, as I happened to be dealing with a case of quartan malaria, whereas Laveran was working in a country where the subtertian parasite was common. Laveran had the luck to see the particular crescent he was observing undergo exflagellation, and was therefore able to recognize that it was indeed a living object and parasitic in nature. These important discoveries depend, therefore, to some extent, upon fortuitous circumstances. Had I had the luck that Laveran had, what a swell I would have been!"[1]

[1] C. L. A. Laveran died at the age of 76 some five weeks after Manson. He was one of the pioneers who, at the end of the

In this connexion we may recall his curious speculation that in certain diseases the carriage of germs pathogenic to man by their intermediary hosts might be reversed, and that man himself might be put to this use by the parasite of some insect or arthropod. As he said: "If an insect can act, as has been abundantly proved, as the carrier of a germ from one man to another, why should not man act in a similar capacity?"

As an originator of lines of research Manson was pre-eminent. Nothing gave him greater pleasure than to suggest research work and lay down the lines on which it should be carried out. He gladly placed his ingenuity and sagacity at the service of others, and his generosity evoked a not less generous response from pupils and associates. Professor R. T. Leiper, for example, one of the most distinguished of them, has acknowledged how much he owed to Manson for counsel and encouragement and for the opportunities of research which were afforded him. "Manson made these opportunities," he once wrote, "while others waited for them to occur." [1]

nineteenth century, blazed the trail of tropical medicine. Born in Paris on 15 June, 1845, the son of a military officer, he was sent in 1878 to Algiers as an Army surgeon. It was in Constantine, on 6 November, 1886, that he first observed the parasite of malaria. In a little book, scarcely more than a pamphlet, he described the various forms of it which he had observed in the blood and illustrated his description with excellent freehand drawings. Subsequently, in 1884, he was appointed Professor of Military Hygiene and Clinical Medicine at Val-de-Grace, and so was deprived of further opportunities of working on his favourite subject. After thirteen years of administrative work he joined the Institut Pasteur as Professor, and there resumed his researches on blood parasites. In 1907 he won the Nobel Prize and became a foreign Fellow of the Royal Society in 1916.

[1] *British Medical Journal*, 19 June, 1926, p. 1050.

Of the form which his suggestions for research work took and the logical order in which they were tabulated, the following example may be cited by one who is not the least grateful of his pupils :

List of Suggestions for Work on Filariasis, written to his Son-in-law about to start for Fiji

(1) Make, say, a hundred observations of native blood, drawn at noon, and again a similar number drawn at night.

(2) Finding some case or cases in whom larvæ occur both day and night, make a three-hourly chart of the findings in a measured amount of blood.

(3) Finding some cases in which larvæ are absent during the day, but present at night, make a similar set of observations.

(4) Of cases in which larvæ occur during the day and not at night, make a similar set.

(5) If you find that there is no periodicity in the filariæ of the Fijian, seek out Indians or Chinese with filariæ and ascertain if periodicity is maintained in them. If so, and it is not maintained in the Fijian, the inference is that there is a Fijian and an Asiatic filaria, which can only be settled by the discovery of adults ; search for the latter.

(6) Be alive to the possibility that the native may have a double infection of the Fijian and Asiatic worms. In making these observations, be careful that the patient is and has been for some time living under the same conditions as regards sleeping and waking. For statistics, always take measured quantities of blood."

Ready as he was to take fire at a new idea, and quick to initiate a new line of investigation, Manson had nothing of the fanatic in his composition. Often it was remarked of his mental make-up that his receptivity of new ideas was balanced by calm and sane judgment and Scottish caution, and these characteristics may be seen running through all he wrote and did. His disposition was to lead rather than to dominate. There was about him, in his presence and bearing, that which

arrested attention and commanded respect. Upon native races his commanding yet benignant presence never failed to make a favourable impression. We have already remarked upon his influence with the Chinese, whom he held in real affection; it was the same with other simple folk: he was not less sympathetic and happy in his contact with Scottish gillies or Irish peasants. Often when speaking of one of his humble friends he would describe him as "just one of Jock Tamson's bairns."

As to his literary style, it was informed by a nice sense of the value of words. He had an extensive vocabulary, he knew how to turn a sentence, he had a happy command of simile and illustration, and in the medical literature of this generation there are few descriptions of symptoms and pathological findings more graphic than those in which his published writings abound. When invited by pupils and colleagues to reveal to them the art of writing scientific papers he would lay down the principle, "Know what you have to say and say it." It was perhaps as good a reply as was possible; but there was much more of conscious art in his own writing than this saying indicates, and of those to whom the counsel was given there were few, probably, who regarded it as fully explaining the effectiveness of his literary style.

APPENDIX

I. DEGREES AND HONOURS

PATRICK MANSON, b. 3 October, 1844, d. 9 April, 1922, in his 78th year.

M.B., C.M. Aberd., 1865. M.D. Aberd., 1866. LL.D. Aberd., 1886.

M.R.C.P. Lond., 1890. F.R.C.P. Lond., 1895.

F.R.S., 1900.

D.Sc. Oxon., 1904. LL.D. Cambridge, 1921.

C.M.G., 1900. K.C.M.G., 1903. G.C.M.G., 1912. "In recognition of the eminent services in connexion with the investigation of the causes and cure of tropical diseases."

LL.D. Hongkong University, 1919.

Consulting Physician to the Seamen's Hospital Society, 1912.

President Epidemiological Society, 1900–02.

First President Society of Tropical Medicine and Hygiene, 1907–09.

Fothergillian Medal, Medical Society of London, 1902.

Mary Kingsley Medal, Liverpool School of Tropical Medicine, 1905.

Bisset Hawkins Gold Medal, Royal College of Physicians, 1905.

Lane Lecturer, Cooper Medical College, San Francisco, 1905.

Corresponding member Académie des Sciences, Institut de France, 1910.

Foreign Associate of the Académie de Médecine, France.

Jenner Medal, Epidemiological Society, 1911, Royal Society of Medicine.

Gold Medal, subscribed by International Committee, International Congress of Medicine, 1913.

Honorary Member of the Société de Médecine de Gand.

Honorary Member of the Manila Medical Society, 1903.

Honorary Fellow of the Society of Tropical Medicine of Cuba, 1908.

Hon. Associate of the Royal Academy of Medicine, Turin.
Associate of Academy of Science of Christiania, 1911.
Honorary Associate of the Medico-Chirurgical Society of Edinburgh, 1921.
Honorary Fellow of the Royal Society of Medicine, 1919.

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